THURSDAY, JANUARY 3, 1878

THE LAST OF THE GASES

THE year 1877 will ever be memorable in the history of scientific progress, its close having been marked by a brilliant series of researches which have ended in an absolute demonstration of the fact that molecular cohesion is a property of all bodies without any exception whatever.

This magnificent work divides itself into two stages, which we shall refer to separately: first the liquefaction of oxygen, and then, following close upon this, the liquefaction of hydrogen, nitrogen, and atmospheric air.

In the liquefaction of oxygen, which we announced last week as having been accomplished by M. Pictet of Geneva, we have not only an instance of the long time we may have to wait, and of the great difficulties which have to be overcome, before a theoretical conclusion is changed into a concrete fact—something definite acquired to science; but also another instance of a double discovery, showing that along all the great lines of thought opened up by modern investigation and modern methods, students of science are marching at least two abreast.

It appears that as early as December 2 M. Cailletet had succeeded in liquefying oxygen and carbonic oxide at a pressure of 300 atmospheres and at a temperature of – 29° C. This result was not communicated to the Academy at once, but was consigned to a sealed packet on account of M. Cailletet being then a candidate for a seat in the Section of Mineralogy. Hence, then, the question of priority has been raised, but it is certain that in the future the work will be credited to both, on the ground that the researches of each were absolutely independent, both pursuing the same object, creating methods and instruments of great complexity. We regret, therefore, that M. Jamin, at the sitting of the Academy to which we have referred, seemed to strain the claims of M. Cailletet by stating that to obtain the gas non-transparent was the same as to obtain it liquefied. We are beginning to know enough of the various states of vapour now not to hazard such an assertion as this. This remark, however, rather anticipates matters, and indeed, as we shall show afterwards, M. Cailletet need not himself be very careful of the question of priority—even if it were ever worth caring for except to keep other people honest.

Owing to the double discovery and the curious incident to which we have referred, the meeting of the Academy on the 24th ult. was a very lively one, as not only was the sealed packet and a subsequent communication from M. Cailletet read, but M. Pictet had sent a long letter to M. Dumas giving full details of his arrangements. MM. Dumas, H. St. Claire Deville, Jamin, Regnault and Berthelot all took part in the discussion, the former admirably putting the work in its proper place by the following quotation from Lavoisier :—

"... Considérons un moment ce qui arriverait aux différentes substances qui composent le globe, si la température en était brusquement changée. Supposons, par exemple, que la terre se trouvât transportée tout à coup dans une région beaucoup plus chaude du système solaire, dans une région, par exemple, où la chaleur habituelle serait fort supérieure à celle de l'eau bouillante ; bientôt l'eau, tous les liquides susceptibles de se vaporiser à des degrés voisins de l'eau bouillante, et plusieurs substances metalliques même, entreraient en expansion et se transformeraient en fluides aériformes, qui deviendraient parties de l'atmosphère.

"Par un effet contraire, si la terre se trouvait tout à coup placée dans des régions très froides, par exemple de Jupiter et de Saturne, l'eau qui forme aujourd'hui nos fleuves et nos mers, et probablement le plus grand nombre de liquides que nous connaissons, se transformeraient en montagnes solides.

"L'air dans cette supposition, ou du moins une partie des substances aériformes qui le composent, cesserait, sans doute, d'exister dans l'état de fluide invisible, faute d'un degré de chaleur suffisant; il reviendrait donc à l'état de liquidité, et ce changement produirait de nouveaux liquides dont nous n'avons aucune idée."

When Faraday in the year 1823 (at the age of 31) began the researches indicated in the last paragraph quoted by M. Dumas, and first liquefied chlorine and then several other gases, he had no idea that he had been anticipated, as he had been, by Monge and Clouet, who condensed sulphurous acid before the year 1800, and by Northmore, who liquefied chlorine in 1805. If the great experimenter were among us now how delighted he would be to see one of the greatest ironmasters of France employing the enormous resources at his disposal at Châtillon-sur-Seine, and a descendant of the Pictet, the firm friend of his great friend De la Rive (who was the first to whom he communicated his liquefaction of chlorine), thus engaged in carrying on the work which he made his own.

The methods employed by MM. Pictet and Cailletet are quite distinct and are the result of many years' preparatory study, as testified by M. H. St. Claire Deville and M. Regnault. It is difficult to know which to admire most, the scientific perfection of Pictet's method or the wonderful simplicity of Cailletet's. It is quite certain that the one employed by the latter will find frequent use in future experiments. We may briefly refer to both these methods.

M. Cailletet's apparatus has already been briefly alluded to in these columns. It consists essentially of a massive steel cylinder with two openings; through one hydraulic pressure is communicated. A small tube passes through the other, the sides of which are strong enough to withstand a pressure of several hundred atmospheres, and which can be inclosed in a freezing mixture. It opens within the cylinder into a second smaller cylinder serving as a reservoir for the gas to be compressed. The remainder of the space in the large cylinder is occupied by mercury. M. Cailletet's process consists in compressing a gas into the small tube, and then by suddenly placing it in communication with the outer air, producing such a degree of cold by the sudden distention of the confined gas that a large portion of it is condensed, a process perfectly analagous to that used to prepare solid carbonic acid by the rapid evaporation of the liquefied gas.

In M. Cailletet's experiment with oxygen it was brought to a temperature of – 29° C. by the employment of sulphurous acid and a pressure of 300 atmospheres; the gas was still a gas. But when allowed to expand suddenly, which, according to Poisson's formula, brings it down to 200 degrees below its starting-point, a cloud was at once formed. The same result has since been obtained without

the employment of sulphurous acid, by giving the gas time to cool after compression. M. Cailletet has not yet obtained, at all events, so far as we yet know, oxygen in a liquid form, as M. Pictet has done; on being separated from its enormous pressure it has merely put on the appearance of a cloud.

M. Pictet's arrangements are more elaborate. He uses four vacuum- and force-pumps, similar to those which were recently exhibited in the Loan Collection of Scientific Apparatus for making ice, driven by an engine of 15-horse power. Two of these are employed in procuring a reduction of temperature in a tube about four feet long containing sulphurous acid. This is done in the following way: the vacuum pump withdraws the vapour from above the surface of the liquid sulphurous acid in the tube, which, like all the others subsequently to be mentioned, is slightly inclined so as to give the maximum of evaporating surface. The force-pump then compresses this vapour, and sends it into a separate reservoir, where it is again cooled and liquefied; the freshly-formed liquid is allowed to return under control to the tube first referred to, so that a complete circulation is maintained. With the pumps at full work there is a nearly perfect vacuum over the liquid and the temperature falls to $-65°$ or $-70°$ C.

M. Pictet uses this sulphurous acid as a cold-water jacket, as we shall see. It is used to cool the carbonic acid after compression, as water is used to cool the sulphurous acid after compression.

This is managed as follows:—In the tube thus filled with liquid sulphurous acid at a temperature of $-60°$ C. there is another central one of the same length but naturally of smaller diameter. This central tube M. Pictet fills with liquid carbonic acid at a pressure of four or six atmospheres. This is then let into another tube four metres long and four centimetres in diameter. When thus filled the liquid is next reduced to the solid form and a temperature of $-140°$ C., the extraction of heat being effected as before by the pump, which extracts three litres of gas per stroke and makes 100 strokes a minute.

Now it is the turn of the oxygen.

Just as the tube containing carbonic acid was placed in the tube containing sulphurous acid, so is a tube containing oxygen inserted in the long tube containing the now solidified carbonic acid. This tube is five metres long, fourteen millimetres in exterior diameter, and only four in interior diameter—the glass is very thick. The whole surface of this tube, except the ends which project beyond the ends of the carbonic acid tube, is surrounded by the frozen carbonic acid.

One end of this tube is connected with a strong shell containing chlorate of potash, the other end is furnished with a stop-cock.

When the tube was as cold as its surroundings, heat was applied to the chlorate, and a pressure of 500 atmospheres was registered; this descended to 320. The stop-cock was then opened, and a liquid shot out with such violence that none could be secured, though we shall hear of this soon.

Pieces of lighted wood held in this stream spontaneously inflamed with tremendous violence.

In this way, then, has oxygen been liquefied at last. But this result has no sooner filled us with surprise than it has been completely eclipsed. On the last day of December, a week after the meeting of the Academy to which we have referred, M. Cailletet performed a series of experiments in the laboratory of the École Normale at Paris, in the presence of Berthelot, Boussingault, St. Claire Deville, Mascart, and other leading French chemists and physicists, using the same method as that formerly employed for oxygen and he then and there liquefied hydrogen, nitrogen, and air!

M. Cailletet first introduced pure nitrogen gas into the apparatus. Under a pressure of 200 atmospheres the tube was opened, and a number of drops of liquid nitrogen were formed. Hydrogen was next experimented with, and this, the lightest and most difficult of all gases, was reduced to the form of a mist at 280 atmospheres. The degree of cold attained by the sudden release of these compressed gases is scarcely conceivable. The physicists present at the experiment estimated it at $-300°$ C.

Although oxygen and nitrogen had both been liquefied, it was deemed of interest to carry out the process with air, and the apparatus was filled with the latter, carefully dried and freed from carbonic acid. The experiment yielded the same result. On opening the tube a stream of *liquid air* issued from it resembling the fine jets forced from our modern perfume bottles.

These more recent results are all the more surprising as, at an earlier stage, hydrogen, at a pressure of 300 atmospheres, has shown no signs of giving way.

These brilliant and important results, though, as we have said, they give us no new idea on the constitution of matter, open out a magnificent vista for future experiment. First, we shall doubtless be able to study solid oxygen, hydrogen, and air, and if MM. Pictet and Cailletet succeed in this there will then be the history to write of the changes of molecular state, probably accompanied by changes of colour, through which these elemental substances pass in their new transformations.

There is a distinct lesson to be learnt from the sources whence these startling *tours de force* have originated. The means at the command of both MM. Cailletet and Pictet arise from the industrial requirements of these gentlemen, one for making iron, the other for making ice.

Why then in England, the land of practical science, have we not more men like MM. Cailletet and Pictet to utilise for purposes of research the vast means at their disposal, or at all events to allow others to use them?

It is also clear that to cope with modern requirements our laboratories must no longer contain merely an antiquated air-pump, a Leyden jar, and a few bottles, as many of them do. The professor should be in charge of a work- instead of an old curiosity-shop, and the scale of his operations must be large if he is to march with the times—times which, with the liquefaction of the most refractory gases, mark an epoch in the history of science.

HUXLEY'S PHYSIOGRAPHY

Physiography: an Introduction to the Sutdy of Na ure. By T. H. Huxley, F.R.S. (London: Macmillan and Co., 1877.)

AMONG educational works which are calculated to afford real assistance to the teacher in his all-important labours, we may recognise two distinct classes. One

of these includes the "text-books," which should aim at presenting only the accurate and well-proportioned outlines of a system of instruction, leaving it to the teacher himself so to fill in these outlines with explanation and illustration, as to cause the new facts and reasonings to produce the most vivid and abiding impressions upon the minds of his pupils. But inasmuch as the attainment of such a result demands much practical skill and educational tact—a skill and tact which are by no means easy of acquirement—the necessity and value of another class of works becomes manifest. This second class of educational works comprises such as aim at instructing the teacher how best to perform his difficult task; which exemplify the work of explanation, illustrate the art of illustration, and show how the dry bones of barren facts may, by clear arrangement and logical connection, be compacted into a body of real knowledge, and this body by being infused with the earnest intelligence of the teacher, may be quickened into active and fruitful life in the minds of the scholars.

It is to this latter very important class of educational works that we should be inclined to refer the book before us, and we cannot therefore regard the designation of it as a "manual for students," which is borne upon its cover—one for which we suspect that the author is not himself responsible—as either happy or judicious. That some instruction in the physical laws of that universe in which we are placed ought to form a recognised part of our system of elementary education has been again and again maintained and strongly insisted upon by scientific men, and by none more persistently or more urgently than by the author of the present work. When we reflect on the fact that to the man who has learnt to recognise, obey and apply these laws, Nature reveals herself as a helpful and bountiful mother, ever ready to aid him n his industry, his arts, and his commerce; while to him who ignores or violates these laws she is known only as a terribly relentless and avenging goddess, ever thwarting his most earnest endeavours, and scourging him with plagues, pestilences, and famines—it is hard to realise how slowly the necessity for this instruction in natural knowledge has forced itself upon the minds of those who are responsible for the scheme of elementary education adopted in this country. But society—the machinery of which is every day becoming more complicated and more susceptible to those painful consequences which follow from the infringement of the laws of nature—will doubtless in the end demand, as indeed it has a right to do, that every unit in her organisation should be fitted so to play his part, as to avoid the danger to himself and others which the neglect or violation of natural laws invariably entails.

Almost every demand that the principles of physical science should be taught in our elementary schools, has been met with the objection that our knowledge of nature and her laws has in recent years grown to such an extent, and ramified into so vast a number of channels as to make any attempt to teach it to the young quite hopeless. As well might we point to the number of volumes in the library of the British Museum, and declare that their existence demonstrates the uselessness of teaching the art of reading. No one, of course, would desire that an epitome all the sciences should be taught to children; but what is demanded is that the methods of modern scientific thought should be made familiar to every mind, that a few leading and necessary truths should be taught concerning the world in which we live and the laws which control its potent forces (seeing that upon our knowledge or ignorance of these depends much of our happiness and success or our misery and failure in the adventure of life), and that, last but not least, the minds of all young people should be conducted within the threshold of the temple of natural knowledge, so that any among them that may be endowed with the necessary capabilities may learn there to dedicate themselves to the pursuit of science.

How can this elementary instruction in science be best imparted to the young? This is the important question which Prof. Huxley applies himself to answer in the work before us; and he accomplishes his object much better by means of example than he could by any amount of discussion of the general principles of the art of teaching. On several other occasions the author has indicated the importance of making a knowledge of the more striking phenomena of nature, those with which we come into contact in our every-day life, and which exercise the greatest influence on our daily occupations and experiences, the starting-point of our introduction to the world of scientific thought; and it is to this vestibule of the temple of natural science that he applies the name of "Physiography."

The author of the present work of course recognises that first principle of good teaching which consists in fastening at first on facts and ideas which are known and familiar, and from thence leading the minds of the student by a succession of steps, no one of which shall present any serious difficulties, up to those more unfamiliar observations and those less obvious deductions from them, which if presented in the first instance might startle and repel rather than attract the scholar. We must ask the reader himself to trace in the work before us how, setting out from the most striking and easily observed facts about the River Thames, Prof. Huxley shows his admirable skill in teaching by leading his readers through a series of reasonings couched in simple and untechnical, but always accurate and elegant, language, up to the grandest truths in physics, biology, geology, and astronomy; how, throughout, happy analogies and telling illustrations make the path of the scholar, light, easy, and pleasant; and how in all this nothing of the exactness and dignity of science is sacrificed to a desire to say those fine or funny things which are too often supposed to convert a prosy book into a "popular" one.

The teacher who takes these easy lessons in elementary science and simply repeats them to his scholars can scarcely fail to communicate some sound and useful instruction to them. But every competent and judicious teacher will prize Prof. Huxley's book rather as a model than as a "crib"—and this is the light in which the author, we are persuaded, would desire that his work should be regarded by them. It is as easy, for example, to make the Mersey, the Severn, the Forth, or the Clyde the starting point of our studies of nature, as the Thames, and in Manchester, Bristol, Edinburgh, or Glasgow respectively, it is far better to do so; nor will any well-instructed teacher find the smallest difficulty in thus adapting his lessons to his

auditory. To such teachers as have never studied or thought on scientific questions themselves, our advice would be to content themselves with placing Prof. Huxley's book in their school-libraries, and not to run the risk of spoiling its teachings by filtering them through their own minds.

We have spoken at such length on the value of this work to the teacher, as to leave but little space for reference to its interest to the general reader, yet this is by no means small; to those who seek an "introduction to the study of nature," which shall be at the same time both sound and readable, exact and untechnical, we most heartily commend the work before us.

We are informed in the preface that the idea of this work has long been entertained, and its plan and methods frequently revolved in the mind of the author. It is probable that not a little of its present excellence is due to this slow maturation of its plan, assisted, as we learn that its development has been, by its embodiment in two successive courses of lectures—on the shorthand notes of one of which the present book is based. In seeking for an editor to relieve him of the more trying labour of book-making, Prof. Huxley has been fortunate in securing the services of Mr. Rudler, whose knowledge of a great number of branches of science is combined with much literary skill. To this cause we may attribute the small number of inaccuracies in either fact or expression which a careful perusal of the work has revealed. Such as do occur may be easily remedied in the new edition, which we have no doubt will soon be called for.

In concluding this notice we cannot refrain from congratulating its author upon the production of the work, and at the same time of assuring him that among all the labours he has undergone, and the sacrifices he has made on behalf of elementary education in this country, none is likely to produce more valuable and more enduring fruit than this much-needed model of the art of teaching the fundamental truths of natural science, the appearance of which at the present time we cannot but regard as being most opportune.

J. W. J.

OUR BOOK SHELF

Myths and Marvels of Astronomy. By Richard A. Proctor. (London: Chatto and Windus, 1878.)

THE author observes in his preface that "the chief charm of astronomy with many does not reside in the wonders revealed to us by the science, but in the lore and legends connected with its history, the strange fancies with which in old times it has been associated, the half-forgotten myths to which it has given birth," and further remarking that in our own times myths and fancies, startling inventions and paradoxes have also found place, he has framed the present volume to meet the tastes of the class of readers which he believes to be specially interested in such matters.

In a work confessedly written with this object in view, perhaps it will hardly be expected that there can be much to require notice in a scientific journal. An important point will be accuracy of detail, and in this respect, except in two or three cases, we remark little to which exception can be taken. Amongst other subjects, the author enters upon "the religion" and the mysteries of the Great Pyramid, "Suns in flames," the rings of Saturn, comets as portents, the notorious lunar-hoax of 1835-36, and the origin of the constellation-figures. He is unlucky in his notice of the first discovery of the famous star of Tycho Brahe in 1572, reproducing from Sir John Herschel's "Outlines," the story of Tycho's attention having been first directed to the object on the evening of November 11, by seeing "a group of country people gazing at a star which he was sure did not exist an hour before." This story is as much a myth as anything in the volume before us, as will be evident to the reader who consults the account of his first observation and of the observations of others given by Tycho himself, and it is strange that the statement which has misled Mr. Proctor should have been continued in the various editions of Sir John Herschel's "Astronomy" since the year 1833. The account given in the chapter "On some Astronomical Myths" of the actual position of the intra-Mercurial planet question is too incomplete to enable the reader to form a competent judgment thereupon, though it may leave him under the impression that there is something mythical about it. Mr. Proctor appears to reject "the idea of wilful deception" on the part of astronomical observers—in which case the mere expression of disbelief in the existence of an intra-Mercurial body or bodies does not assist explanation of recorded observations, more particularly where motion has been remarked. There are a few numerical errors in the volume, as in the note on p. 235, where it is stated that the comet of the August meteors has "a period of at least 150 years;" so long a period would be irreconcilable with the observations, and the very complete investigation by Prof. Oppolzer assigns 121½ years as the most probable length of the revolution. But as already stated there is general accuracy of detail, and the volume will doubtless be found acceptable to the particular class of readers for whom it has been prepared.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Electrical Experiment

THE inclosed letter gives an account of an experiment in which an electric current appears to be produced by the direct action of gravity, a result which, if clearly established, would be new and of considerable scientific interest.

In trying to repeat the experiment yesterday I observed a considerable deflexion of the galvanometer in the direction described by Mr. Pirani, but as this deflexion seemed to occur some seconds *after* the inversion of the tube, I examined the tube and found a small bubble of air working its way up through the solution, and as soon as it came to the top of the tube the deflexion occurred.

I have not yet had time to repeat the experiment without the bubble, but I mention this to show that care must be taken to secure that the electrolyte is homogeneous, and that it does not contain anything which will either sink to the bottom of the tube or float to the top, so as to act alternately on the two electrodes.

The fact that the deflexion continued for some time after the tube was placed horizontally seems to indicate the possibility of something which was shifted from end to end when the tube was inverted, but remained where it was when the tube was only laid on its side.

J. CLERK MAXWELL

Cavendish Laboratory, Cambridge, December 28, 1877

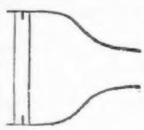
"University of Melbourne, Oct. 30, 1877

"MY DEAR SIR,—On page 317 of vol. i. of your 'Electricity and Magnetism' it is pointed out that a greater electromotive force is required to produce a given current between zinc electrodes in a solution of sulphate of zinc when zinc is carried upwards than when it is carried downwards.

"I am not aware that it has been noticed that by the same

reasoning as that by which the induction of currents is deduced from the force exerted between a circuit and a magnet and the existence of contact electromotive force from the Peltier effect, it follows that a current should exist if two zinc electrodes connected by a wire are immersed in a solution of sulphate of zinc, the direction of the current being (in the solution) from the upper to the lower electrode.

"I tested this a few days ago, using a glass tube eighteen inches long, filled with a saturated solution of sulphate of copper and closed by copper caps with wires attached.



"On connecting the wires with a very delicate Thomson's astatic galvanometer belonging to Prof. Halford, a very considerable deflection was produced (200 divisions) when the tube was held vertically, the direction of the deflection being reversed when the tube was reversed.

"If the tube, after being held vertically, was placed in a horizontal position, the deflection diminished, but several minutes elapsed before the index came to zero, which it eventually did. I cannot explain the time taken. I am now preparing to test the actual loss of weight of the upper electrode.

"I have the honour to be, Sir,
"Your obedient servant,
"F. J. PIRANI,
"Lecturer on Natural Philosophy and Logic,
University of Melbourne.

"P.S.—If the phenomenon has not been noticed before I shall be obliged if you will kindly communicate it to NATURE.
"F. J. P."

The Telephone

I HAVE been much interested in the communication by Dr. Röntgen on a telephonic alarum. During the past six or seven weeks, in investigating the phenomena of the telephone, chiefly as to the suggestions they offer regarding the mechanism of nervous transmission, I have frequently shown to friends the striking experiment described by Dr. Röntgen, and, amongst others, to Sir William Thomson. It has succeeded with Ut_2, Ut_3, and with numerous forks up to Ut_5, but, as stated by Dr. Röntgen, the best result was obtained with Ut_4. With those below this pitch the tone was feeble, whilst with those above it it was transient, in consequence of the difficulty of keeping the small fork going. With Ut_2, worked continuously by an electro-magnet, another fork of the same pitch sounded loudly and steadily. I have also been engaged in some endeavours to record on a moving surface the vibrations of the plate. These have been so successful as to show that it is only a question of delicate adjustment. In endeavouring to utilise one telephone by making several friends listen at once, I have found that by fixing the metal disc to a thin membrane over a small cavity filled with air, like a Koenig's capsule, and having a number of flexible leaden tubes connected with it, an ear placed at the end of each tube will hear distinctly. JOHN G. MCKENDRICK

Physiological Laboratory, University of Glasgow,
December 31, 1877

The Radiometer and its Lessons

PROF. OSBORNE REYNOLDS (vol. xviii. p. 121) appears to have done himself less than justice in the extracts he has sent you from his earlier papers, as representing his published views on the action of residual gas in radiometers. For the extracts do not suffice to constitute an explanation of this action, whereas the papers from which he makes the extracts contained what, if true, might have been an explanation of the action of residual gas, along with much else that is admittedly erroneous; and although those papers (the only ones published before mine) conclude with Prof. Reynolds's own expression of opinion that residual gas is not the cause of the force observed by Mr. Crookes.

He quotes three paragraphs. In two of these he recited the fundamental principle in the kinetic theory of gases which he sought to apply. To obtain an explanation of the phenomenon from this principle according to the method pursued by Prof. Reynolds, it was necessary for him (*a*) to establish a law connecting an excess of force perpendicular to the disc with a flow of heat in radiometers, and (*b*) to indicate agencies which could occasion a sufficient flow of heat. He quotes the passage in which he announced the result of his, as I believe, unsuccessful attempt to accomplish the former of these, but he omits the equally necessary passage in which he dealt with the latter. It will be found at page 407 of the *Proceedings* of the Royal Society, vol. xxii., and is couched in the following terms:—"It must be remembered that ϵ [which measures the outflow of heat] depends on the rate at which cold particles will come up to the hot surface, which is very slow when it depends only on the diffusion of the particles of the gas *inter se*, and the diffusion of the heat among them. It will be much increased by convection currents." If this passage, as was requisite, had been added to the extracts made by Prof. Reynolds, it would have brought his recent account of the views he had announced into conformity with my account of them.

In connection with this subject it should be observed that Prof. Osborne Reynolds has in express terms excluded from his explanation that which I believe to be the real agency which brings a sufficient supply of cold molecules up to the hot surface, for he states, in his letter to NATURE (vol. xvii., p. 27), that "it is incompatible with his explanation that the increase resulting from rarefaction in the mean length of the path of the gaseous molecules would favour the action." Now the polarisation of the gas depends on the ratio which this mean length bears to the interval between heater and cooler.

I cannot find anywhere in Prof. Osborne Reynolds's writings an explanation of the thing to be explained, viz., that the stress in a Crookes's layer is different in one direction from what it is at right angles to that direction. Let v be the component of the momenta of the molecules striking a square unit of the heater in the unit of time, resolved perpendicularly towards the heater; and let u be the corresponding normal component of their momenta from the heater, when they are thrown off. Then $u + v$ is the pressure on the heater. Now if u and v could result respectively from *unpolarised* motions in the gas, the momentum resolved parallel to the heater would be $\frac{1}{2}u + \frac{1}{2}v$ from left to right, with an equal momentum from right to left. Adding these we find $u + v$ the pressure of the gas parallel to the heater. This is equal to the normal pressure, and, therefore, under these circumstances, there would be no Crookes's force whatever. It is only when we take the polarisation of the gas into account that the momenta resolved parallel to the heater become different from $\frac{1}{2}u$ and $\frac{1}{2}v$.

Prof. Osborne Reynolds says that my views are at variance with results arrived at by Clausius and other discoverers in this branch of physics. I do not myself value appeals to authority in matters of science. But it so happens that here again it appears to be Prof. Reynolds who makes the mistake. Clausius, in his great memoir on the conduction of heat by gases, published in 1862 (*Phil. Mag.*, vol. xxiii. p. 529), warns his readers against the very error into which Prof. Reynolds seems to fall, and points out that there "are obvious limits" beyond which the laws he had discovered for the conduction of heat do not prevail, one of which limits is that the gas "must not be so expanded that the mean length of excursion of the molecules becomes so great that its higher powers cannot be neglected." Now it is just to this excepted case, to the Sprengel vacua experimented on by Mr. Crookes, that Prof. Osborne Reynolds applies the laws of conduction, and he then objects to my theory that it does not agree with the laws so misapplied. The phenomenon of Crookes's stress appears to come into existence precisely in Clausius's excepted case, viz., so soon as the ratio which the mean length of excursion of the molecules bears to the interval between heater and cooler, is such, that when multiplied by a function of the temperatures of the heater and cooler, its square is of appreciable magnitude in Clausius's equations. This may be experimentally secured either by placing the heater and cooler very close together, as in experiments upon spheroidal drops, or by excessively attenuating the gas so as to lengthen the free paths of the molecules sufficiently, as in radiometers. G. JOHNSTONE STONEY

Dublin, December 20

POSTSCRIPT, December 22.—I have just seen Prof. Schuster's letter (NATURE, vol. xvii. p. 143). Dr. Schuster will pardon me if I say that he has adopted a scarcely legitimate course in introducing into a discussion on priority his present reminiscence of one of the conversations about the radiometer which he held with his friend, Prof. Osborne Reynolds, two and a half years ago. The language in which he reports it is foreign to Prof. Reynolds's style of composition, so that we may conclude we are dealing with Dr. Schuster's words, and the words which occurred to him after he had read much else on the subject. No

judicially-minded person would attach much weight even to a report of his own, drawn up under such circumstances, and all judicially-minded persons will regret its introduction here. Prof. Osborne Reynolds's reasoning proceeds on the hypothesis that the gas is not polarised. The only real question here is, Is Prof. Schuster prepared to maintain that this reasoning is correct?

Prof. Schuster, in reporting his reminiscences, first recites a kinetic principle which is quite consistent with there being as much force sideways as perpendicularly to the disc, and which therefore contains no explanation of the phenomenon; and when he comes to the first essential point, viz., that which requires him to show that "an increased pressure on the cold side of the vanes of a radiometer will not counterbalance the force acting on the blackened sides," all that he has to say on the subject is that "he does not think that such is the case!" This is the essential thing to be *proved* before the explanation can be accepted, and he recites experiments which show that it is essential.

Prof. Schuster concludes this part of his letter with the admission that "he does not see how [on his theory] an increase in the force can take place" as the exhaustion proceeds. So much the worse for the theory, since experiment indicates that such an increase in the force does take place. In proof of this I may allege, in addition to Mr. Crookes's experiments, several series of experiments made by Mr. Moss, one of the most judicially-minded, patient, and dexterous manipulators I have met with. The experiments were made with the apparatus described in a communication from him and myself, published last spring in the *Proceedings* of the Royal Society, and the effect of the convection current was with extreme care excluded in two ways—by placing the swinging disc where the influence of the convection current on it before and behind was balanced, and by observing the motions that arose before the convection current had time to reach the disc. Both methods concurred in showing that, as in Mr. Crookes's experiments, the force on the disc uniformly increased with increasing attenuation of the gas up to the limit to which we pushed the exhaustion. Mr. Crookes has shown that beyond that limit it begins to decrease. Prof. Schuster will do a real service to science if he will devote his great skill for some months to repeating these and other concurrent experiments, and either confirm them or point out why they should be set aside.

Prof. Schuster thinks that "any theory of the radiometer which makes the action depend on the comparatively large [? small] ratio of the mean free path to the dimensions of the vessel, must necessarily be wrong." Has not Prof. Schuster here overlooked the minuteness of the phenomenon which has to be accounted for? Spheroidal drops of less than a millimetre diameter are easily formed of several light liquids. The Crookes's stress which supports these is an excess of vertical stress over horizontal stress in the supporting layer of polarised gas, amounting to about the 12- or 15-thousandth part of the whole stress. This compares favourably with the minute ratio to which Prof. Schuster refers.

I will not at present enter on that part of Prof. Schuster's letter in which he criticises my published views on penetration, as he refers me to the researches of Messrs. Kundt and Warburg, which I have not yet seen. G. JOHNSTONE STONEY

Glaciation of Orkney

LAST spring Prof. Geikie informed me of a correspondence which was going on in the columns of NATURE as to the question whether the Orkney Islands bore evidence of having been glaciated. It was with much surprise that I heard that there could be any question on this point at all, but I refrained from submitting my opinion to the public—unhesitating though that opinion was—on account of my being then just about starting for my native county, and thus having an opportunity of very specially directing my attention anew to the matter. As the observations I then made *without exception* tended to confirm me in what really required no confirmation, I think I may now come forward as one who has for long known those islands, and who has made a very special geognostic survey of them, during many years. And I would first say, as regards the question, "whether Orkney does or does not give proof of having been covered by a great ice-sheet?" that I believe that no one who has educated his eye—not by looking at pictures in books, but among the rocks themselves—to the *apprehension* and recognition of the hill-contours of an ice-scalped country, would hesitate to declare Orkney to be such. Let such a one take his stand, at a sufficient altitude, anywhere along the north coast of Sutherland, with a scratched and polished boss under his feet, rolling up into rounded hillocks on every side, and sweep his eye from the two Ben Griams over to Hoy, and he could not but exclaim, "*There* is a country which has suffered sore."

In having to differ from Mr. Laing, I join issue with him on two points—boulders and foreign stones, and boulder-clay. I have also to corroborate Prof. Geikie as to glaciation near Stromness; for I, during last summer, saw to the immediate north-west of Stromness a surface of gneiss, say ten feet by three, most unmistakably glaciated—both scratches and polishing being shown.

Now as regards "boulders and foreign stones." Mr. Laing will find—I wonder at his not knowing of it—about 100 yards to the west of the House of Saval, in Sanday, one of the finest boulders in Scotland. This boulder, of great size, consists of hornblendic gneiss; for long I was unable to identify it with any variety of the hornblendic gneiss of Sutherland; but this very year's work enables me to say that it is *very similar* to that of a locality near Durness. In all probability, however, its parent rock lay east, not west.

Another boulder I have heard of, but not seen; it was described to me under the name of the "Eagle Stone"; it lies upon the side of a hill in Westray, near Pierowall, and is said to be peculiar as a loose stone, both on account of its toppling position, its being different from any rock in Orkney, and from there being no rock near it.

As to there being "foreign stones" in Orkney, I shall only say that I have at present in my collection polished jaspers, picked up in rolled masses in Orkney; and that fragments of broken agates are found not unfrequently, on the hill tops and sides, in Hoy. These are quotations, *ex grege*.

Mr. Laing's very precise observations on the *clay beds*—let us call them—do call for special investigation.

If the conclusions arrived at by other observers are found to coincide with his—while they could not affect the ultimate decision as regards the ice-clad country—they certainly would strike these clay beds out of the category of boulder-clays. But, sir, I have seen these clays, and I did not see what Mr. Laing saw; and what I did observe leads me to doubt the correctness of his conclusions. For I found it to be a notable circumstance as regards these Orkney clay-beds that they are very markedly *clay*-beds; *i.e.*, that the amount of clay relatively to that of the stoney matter therein is very much greater than that of most boulder-clays.

Now this is a fact which saps the very foundation of Mr. Laing's observation—an abnormally argillaceous clay bed being the result of the disintegration of a normally siliceous sandstone is difficult to conceive. Nay more, although the cement of certain of the Orkney beds is silicate of alumina, forming the blue argillaceous flag, it is an unquestionable fact, that these flags do not disintegrate by the action of the weather. Even the Picts knew that when they built their Broughs thereof. Silicate of alumina is not affected by carbonated waters.

Upon—nearly all along—the west shore of Shapinsha there are cliffs—sea-cliffs of these clay beds, which lie *betwixt the rocks*, or the last visible rock, *and the sea*; that last rock is a red ferruginous loose-grained sandstone, with little or no cement, what there is being micaceous; the clay beds are ochre yellow. The disintegration of this rock never could have yielded these clay beds.

But Mr. Laing may argue that they resulted from the decay of an overlying argillaceous bed. The argument will not stand. Firstly, because the dip is the wrong way; the rock dips at a high angle to the east; the clay *slightly* caps it, and stands as a bank between its escarpment and the sea. Secondly, because a friable yellow freestone, destitute of argillaceous cement, should overlie the red beds. Thirdly, because on the other side of the bay where the argillaceous flags do appear they are quite permanent. Ice might *grind them up*—the "weather" does not rot them down. But here no clay beds are seen.

Finally, sir, I would request my talented countryman—whom I have great pleasure in breaking a lance with in this field—to consider how or why it is that these clay beds are found only *on one side* of the long depression which runs up the centre of the islands? M. FORSTER HEDDLE

University, St. Andrews, December 19, 1877

Northern Affinities of Chilian Insects

I THINK I may be allowed to express my surprise at Mr. McLachlan's statement that this subject has never yet been

"even more than casually alluded to in works on geographical distribution," and is "*ignored in the principal ones*;" when I have devoted no less than six pages of my book on "The Geographical Distribution of Animals" (vol. ii. pp. 42–48) to a discussion of the main facts—quite as much as could be properly given to it in a general work. It is, however, well worthy of a detailed study, which I am very glad is being undertaken by so competent an entomologist. I hope Mr. McLachlan will endeavour to obtain collections of coleoptera and other orders of insects from the higher tropical Andes, where, I feel confident, some northern forms will also be found.

ALFRED R. WALLACE

Mr. Crookes and Eva Fay

A FEW words from myself seem to be called for by the recent letter of Mr. Crookes in reply to Prof. Carpenter, published in your journal. As far as I am concerned, the breach of etiquette complained of can only apply to my obtaining the publication of the letter Mr. Crookes addressed to me in the *Banner of Light*. The subsequent *facsimile* that appeared I am not in any way responsible for.

The part I took in the matter is very simple, and may be briefly explained as follows:—On Mrs. Fay's return from England to this country the genuineness of her mediumship was very much doubted, and was the subject of controversy not only in the spiritual journals, but in other papers as well. Having, whilst in England, satisfied myself that the manifestations were real, I defended her to the best of my ability, and on seeing it stated in the *Boston Herald* that Mr. Crookes had withdrawn his confidence in her, I thought it prudent to write to that gentleman, mentioning my reason for so doing. In due time I received a courteous reply, which I at once took to the *Banner* office, never dreaming that Mr. Crookes could have any possible objection to its publication after the articles he had himself published on the subject in the English journals. Months elapsed, when one day to my surprise I met with the *facsimile* letter in the *New York Daily Graphic*. On mentioning the subject to the editor of the *Banner* he also expressed surprise, and stated his inability to account for the publication of the *facsimile*. He at first was of opinion that I had taken the letter away and mislaid it; but on searching, the document was subsequently found in the office. Hereupon both Mr. Colby and myself wrote to the editor of the *Graphic*, requesting him to state how he obtained possession of the original letter, so as to get the *facsimile* prepared; but neither of us received a reply. I then got a gentleman residing in New York to call on the *Graphic* editor on the subject, and was informed that the said editor declined to say how he obtained possession of the letter. Thus the matter stands, and is as inexplicable to-day as it was at the time it happened.

I entirely exonerate the editor of the *Banner* and his associates from any complicity in the matter, and I trust Mr. Crookes, after this explanation, will see that his imputation against American honour is wholly unfounded.

The publication of the letter in the *Banner* I alone am answerable for; and as I explained in my letter to Mr. Crookes that my object was to meet a statement in a public journal, I of course thought that he must have felt that the reply he forwarded would in all probability be made public use of.

Boston, U.S.A., December 7, 1877 ROBERT COOPER

P.S.—Mr. Crookes errs in speaking of me as "a Boston gentleman." I am an Englishman temporarily located here.—R. C.

Philadelphia Diploma

IN NATURE, vol. xvii. p. 153, it is stated that "A 'Dr.' Harmuth, in Berlin, who received his diploma from Philadelphia, was lately sentenced to pay 300 marks for using the prefix publicly." It is but just to so old and respectable a university as that of Philadelphia to point out that "Dr." Harmuth's diploma could not have been genuine. So-called "Philadelphia degrees" of all sorts are sold by agents, but they have no connection with the University of Philadelphia, nor have they, at present, any connection with the city, though the author of this scandalous imposition once lived there and carried on a disreputable practice as a quack doctor. The public should still be on their guard against Bogus degrees, for diplomas purporting to issue from several American and German universities are still to be had, in some cases on examination *in absentia* and payment of the fee, in others by a money payment only. C. M. INGLEBY

Valentines, December 26, 1877

Royal Dublin Society

IN justice to myself I beg to state that my function as editor of the Natural Science papers in the "Scientific Proceedings of the Royal Dublin Society" begins only with Part 2 of that journal, and that I had no knowledge whatever of the material contained in Part 1 until it had been printed and circulated. By publishing this I shall be greatly obliged.

ALEX. MACALISTER

Anatomical Museum, Trinity College, Dublin

The Meteor of November 23

I HAVE just seen Capt. Tupman's letter in NATURE (vol. xvii. p. 114). I can give a fairly accurate estimate of the direction of the meteor from Llandudno at the time it burst. Sitting in a lighted room my eye was attracted by a bright bar of light across the hearth-rug similar in shape to a gap in the Venetian blind caused by a broken tape. The light slowly faded out in about the same place, which was easily remembered. I listened intently for a report for perhaps about a minute, gave it up, and then heard what was somewhat like the report of a ship's gun at a short distance.

It was easy afterwards to estimate the direction of the light as two points west of (true) north, and thirty-five degrees above the horizon.

I regret that the time between the fading of the light and the report I can only guess very roughly. It may have been about two minutes. T. S. PETTY

51, Boundary Road, N.W.

THE SUN'S MAGNETIC ACTION AT THE PRESENT TIME

PERHAPS no result in magnetism has excited so much interest as that which has connected the varying diurnal oscillation of the magnetic needle, and the frequency of the aurora polaris, with the spotted area of the sun's surface, in a common cycle of ten and a half years. Various investigations have been undertaken in order to determine whether other phenomena could not be found which would take a place in this chain.

That the movements of the magnets and the corruscations of the aurora are due to the cause which produces the immense chasms in the sun's envelopes there can be little doubt; but we know nothing of the mode in which the sun acts on our earth to produce these effects, and we have reason to believe that this ignorance has prevented us hitherto from tracing to the same cause atmospheric variations which have been attributed altogether to the solar heating action.

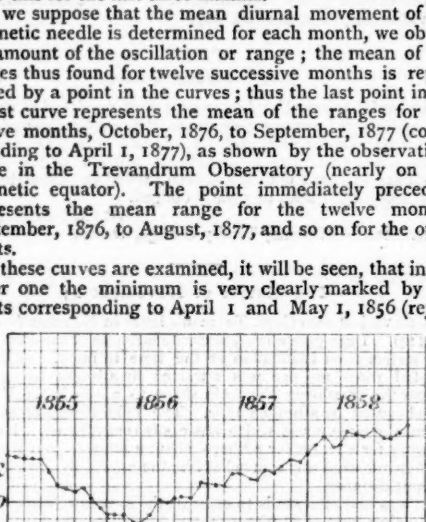
Any facts, then, as to what the sun is doing at the present time with the earth's magnetism will not be without value, whether we regard the facts alone, or as connected with their hypothetical relations to atmospheric phenomena. It should always be remembered, however, that the variations of magnetic oscillations in the decennial period, shown at any one station on the earth's surface, agree generally with those shown over the whole globe, while the meteorological phenomena are so much affected by conditions of position that it is difficult to distinguish what is due to local and what to cosmic causes.

It is well known to those who have studied this subject, that the interval from the time when the sun has fewest, till that when he has most, spots has been less than that from the maximum to the minimum; and that the same fact has been observed in the case of the magnetic oscillations. The way in which the changes of the latter occur near the times of successive minima has not, how-

ever, been studied. The accompanying diagram will show this for the last three minima.

If we suppose that the mean diurnal movement of the magnetic needle is determined for each month, we obtain the amount of the oscillation or range; the mean of the ranges thus found for twelve successive months is represented by a point in the curves; thus the last point in the lowest curve represents the mean of the ranges for the twelve months, October, 1876, to September, 1877 (corresponding to April 1, 1877), as shown by the observations made in the Trevandrum Observatory (nearly on the magnetic equator). The point immediately preceding represents the mean range for the twelve months, September, 1876, to August, 1877, and so on for the other points.

If these curves are examined, it will be seen, that in the upper one the minimum is very clearly marked by two points corresponding to April 1 and May 1, 1856 (repre-



senting the mean ranges, October, 1855, to September, 1856, and November, 1855, to October, 1856), and that there is little difference in the rapidity with which the curve descends to, and ascends from, the minimum.

In the middle curve the epoch of minimum is by no means so distinctly marked; it occurs between the points for April 1 and September 1, 1866. There is also a considerable difference in the rapidity of variation in the descending and ascending branches of the curve. The descent is nearly as rapid as in the upper curve, but the ascent is very much slower.

In the lower curve, the lowest point is that for December 1, 1875, but it is even now, with points for a year and a half later, difficult to say whether this is the minimum or not, the point for January 1, 1877, being only 0'.02 (two-hundredths of a minute of arc) higher. In this curve the change of range in the diurnal oscillation is quite insignificant from November 1, 1874, to April 1, 1877, including the ranges from May 1, 1874, to September 30, 1877, an interval of three years and five months. If this result is confirmed by other observations, as I believe will be the case, no such constant state of the sun's magnetic action will have been observed since the last years of the eighteenth century.

The observations of sun-spots, even if they give as accurate a measure of the intensity of the cause as that obtained from the movements of our magnets, cannot be observed with the same continuity, nor be measured with the same precision; but I have little doubt they will confirm generally the result shown in the last curve, as they have in preceding cases.

With regard to the aurora borealis, the appearances seem to have been very rare during the last two winters. In the report by Capt. Sir G. Nares, on the Arctic expedition, he says that in the winter of 1875-76, "Light flashes of aurora were occasionally seen on various bearings, but most frequently passing through the zenith; none were of sufficient brilliancy to call for notice. The phenomena may be said to have been insignificant in the extreme, and, as far as we could discover, were totally unconnected with any magnetic or electric disturbance" (NATURE, vol. xv. p. 35).

In the twelve months including September, 1843, and August, 1844, including the epoch of minimum disturbance and of auroral frequency, I observed in the south of Scotland (in lat. 55° 35′) thirty appearances of the aurora, and from September, 1844, till the end of 1845, fifty-nine appearances were observed at this single station.[1] Making every allowance for the continuous watch over the magnetic instruments at the Makerstoun Observatory during these years, the difference between Capt. Sir G. Nares' result, in so high latitude, in 1875-76, and that for the south of Scotland, is very distinct. I ought to add, with reference to the apparent want of connection of the faint auroral appearances with the magnetic disturbance noticed by Sir G. Nares, that several of the auroræ observed by me were of the very faintest kind, mere "traces," as I have termed them, which I could never have remarked had I not been warned by *very slight* magnetic irregularities to examine the sky with the greatest attention. Again, in no case have I seen the faintest trace of an aurora without finding at the same time a corresponding irregularity in the movement of the force or declination magnet.

I am unacquainted with any observations of the aurora made in the British Isles during the last two winters;[2] I believe that no scientific institution exists in this country which makes the look-out for aurora throughout the night a definite portion of its work, and that all our knowledge of this phenomenon appears to be left to the chances of some one being out, at the hour of a display, sufficiently bright to attract his attention who will take the trouble to communicate his observation to a public journal.

JOHN ALLAN BROUN

P.S.—I have to thank Mr. A. Buchan for kindly furnishing me with a note of the auroras seen at the stations of the Scottish Meteorological Society during the year 1876. These amounted to forty-two in number, twenty-six in the first half and sixteen in the second half of the year. The greater part were seen in the most northerly stations, including the Orkney, Shetland, and Farö Islands; nine only having been seen south of the Forth. I cannot, however, compare the total result from the hundred stations of the Society with that from the single southerly station of Makerstoun in 1844, since much depends on the nature of the watch kept in each case. It is, however, gratifying to find that so much attention is given at the stations of that highly useful scientific body, the Scottish Meteorological Society, to the observation of this phenomenon.

December 31, 1877

[1] "General Results of the Makerstoun Observations," p. lxxv., *Trans. Roy. Soc. Edin.*, Part 2, vol. xix.

[2] I do not omit Mr. Kinahan's account of "auroric lights," which he saw so frequently in the winter of 1876-77, and which he considered a species of aurora borealis (NATURE, vol. xv. p. 334), as I think there must have been some mistake as to the nature of those lights. He says they were "very common and brilliant during 'the dark days' of December, a few hours before dawn (about five o'clock)." The aurora borealis is very rarely seen at five A.M. in this country. In the two years, 1844 and 1845, during which the aurora was sought for at Makerstoun every hour of the night, it was observed on seventy-seven nights on an average of nearly three hours each night, but it was seen only twice so early, and that with a bright or brilliant aurora which remained during five hours on the first occasion, and from six P.M. to six A.M. on the second. I cannot say, also, that I have ever seen parts of the phenomenon described by Mr. Kinahan, and I had hoped that some other observer in Ireland would have confirmed his observations, which if exact, would be most important, especially as made so frequently at the epoch of minimum.

THE "CHALLENGER" IN THE ATLANTIC[1]

II.

IT still seems but the other day when every zoologist believed with Edward Forbes that not very far below the surface of the sea there existed a region where life was unknown, or where at the most, if it existed it showed but a few sparks, which only served "to mark its lingering presence;" and yet even when Forbes was writing thus, Sir John Ross had brought up from some 800 fathoms deep in Baffin's Bay, "a beautiful *Caput medusæ*," and the present president[2] of the Royal Society had written (August 31, 1845), "It is probable that animal life exists at a very great depth—in the ocean." "On one occasion, off Victoria Land, between the parallels of 71° and 78° S.L., the dredge was repeatedly employed, once with great success at 380 fathoms," and "on another occasion the sounding-line brought up distinct traces of animal life from a depth of 550 fathoms." The history, however, of the subject, is to be found recorded in Sir Wyville Thomson's "Depths of the Sea," and we only here refer to it to remind the reader how completely changed are the general ideas on this subject; and we learn without surprise that "the most prominent and remarkable biological result of the *Challenger's* voyage is the final establishment of the fact that the distribution of living [animal] beings has *no depth limit*, but that animals of all the marine invertebrate classes, and probably fishes also, exist over the whole of the flora of the ocean;" but although life is thus universally extended, probably the number of species as well as of individuals diminishes after a certain depth is reached. This distribution of animal life depends in a marked degree either upon the nature of the sea-bottom or upon the conditions which modify the nature of that bottom. The fauna at great depths was found to be remarkably uniform, and the distribution area seemed to depend mainly on the maintenance of a tolerably uniform temperature. It is curious to note that the families which are peculiarly characteristic of the abyssal fauna, contain a larger number of species and individuals, and these are larger and more fully developed in the Antarctic Ocean, than they are in the Atlantic and the North Pacific.

Though the task of determining the various animal forms procured will occupy a number of specialists for several years, still we have several glimpses of the riches of the ocean fauna in these two volumes. Among these the pretty Hexactinellid sponges, the stalked crinoids, and the echinoids seem to hold foremost places. The stalked crinoids with their lily-like forms are the most remarkable of these, not only on account of their extreme rarity, but also on account of the special interest of their relation to many well-known fossil forms. Of one of these fine forms we give the accompanying illustration (Fig. 3). It was

FIG. 3.—*Pentacrinus maclearanus*, Wyville Thomson. Slightly enlarged.

dredged from a depth of about 400 fathoms, near the Island of San Miguel. It belongs to the genus *Pentacrinus*, and has been called after Capt. Maclear, R.N., the commander of the *Challenger*. The lily-shaped head is about 3½ inches in height, and the stalk may have been several inches longer. The scientific description of such a form must necessarily be very technical, and not easily to be understood by the general reader, who, however, cannot fail to get a correct idea of its general form and

[1] "The Voyage of the *Challenger*. The Atlantic: a Preliminary Account of the General Results of the Exploring Voyage of H M.S. *Challenger* during the Year 1873 and the Early Part of the Year 1876." By Sir C. Wyville Thomson, Knt., LL.D., F.R.SS. L. and E., &c., Regius Professor of Natural History in the University of Edinburgh, and Director of the Civilian Scientific Staff of the *Challenger* Exploring Expedition. Two volumes. Published by Authority of the Lords Commissioners of the Admiralty. (London: Macmillan and Co., 1877.) Continued from p. 148.

[2] Sir Joseph Hooker, C.B.

appearance from the illustration. The special volume in which the whole group of these lily-like starfish will be described is, we understand, to be from the pen of Sir Wyville Thomson.

Though the zoological treasures obtained by dredging were often very great, yet sometimes this often prolonged operation ended in utter disappointment; for example:—The vessel was on her way from Bahia to the Cape, when, on October 2, "we saw our first albatross sailing round the ship with that majestic careless flight which has been our admiration and wonder ever since; rising and sinking, and soaring over us in all weathers, utterly regardless of the motion of the ship, and without the slightest apparent effort. I have often watched these glorious birds for hours from the bridge, and notwithstanding all we know or think we know about the mechanics of flight, to the last I felt inclined to protest that for so heavy a bird to support itself motionless in the air, and perform its vigorous evolutions without a perceptible movement of the wings, was simply impossible by any mechanical means of which we have the least conception.

"On the 3rd we sounded in 2,350 fathoms with a bottom of red mud, still due apparently in a great degree to the South American rivers, and a bottom temperature of 0°·8 C. The trawl was lowered, and on heaving in, it came up apparently with a heavy weight, the accumulators being stretched to the utmost. It was a long and weary wind-in, on account of the continued strain; at length it came close to the surface, and we could see the distended net through the water; when, just as it was leaving the water, and so greatly increasing its weight, the swivel between the dredge-rope and the chain gave way, and the trawl with its unknown burden quietly sank out of sight. It was a cruel disappointment—every one was on the bridge, and curiosity was wound up to the highest pitch; some vowed that they saw resting on the beam of the vanishing trawl the white hand of the mermaiden for whom we had watched so long in vain; but I think it is more likely that the trawl had got bagged with the large sea-slugs which occur in some of these deep dredgings in large quantity, and have more than once burst the trawl net."

FIG. 4.—*Cladodactyla crocea*, LESSON. Stanley Harbour, Falkland Islands. Natural size.

Among the interesting creatures met with living, not in the depths of the sea, but in this instance living amid the fronds of one of the larger algæ, was a Holothuroid, of which we have the following account:—

"The weather while we were at the Falklands was generally cold and boisterous, and boat-work was consequently uncomfortable and frequently impracticable, except in the shallow water within the harbour; we had, however, two or three days' dredging in the pinnace, and made a pretty fair account of the submarine inhabitants of our immediate neighbourhood. *Macrocystis pyrifera*, the huge tangle of the Southern Seas, is very abundant in Stanley Harbour, anchored in about ten fathoms, the long fronds stretching for many yards along the surface and swaying to and fro with the tide. Adhering to the fronds of macrocystis there were great numbers of an elegant little cucumber-shaped sea-slug (*Cladodactyla crocea*, Lesson, sp.), from 80 to 100 mm. in length by 30 mm. in width at the widest part, and of a bright saffron-yellow colour. The mouth and excretory opening are terminal; ten long, delicate, branched oral tentacles, more resembling in form and attitude those of *Ocnus* than those of the typical *Cucumariæ*, surround the mouth; the perisom is thin and semi-transparent, and the muscular bands, the radial vessels, and even the internal viscera can be plainly seen through it. The three anterior ambulacral vessels are approximated, and on these the tentacular feet are numerous and well developed, with a sucking-disc supported by a round cribriform calcareous plate, or more frequently by several wedge-shaped radiating plates arranged in the form of a rosette; and these three ambulacra form together, at all events in the female, a special ambulatory surface.

"The two ambulacral vessels of the 'bivium' are also approximated along the back, and thus the two interambulacral spaces on the sides of the animal, between the external trivial ambulacra and the ambulacra of the bivium, are considerably wider than the other three; consequently, in a transverse section, the ambulacral vessels do not correspond with the angles of a regular pentagon, but with those of an irregular figure in which three angles are approximated beneath and two above. In the female the tentacular feet of the dorsal (bivial) ambulacra are very short; they are provided with sucking-discs, but the calcareous support of the suckers is very rudimentary, and the tubular processes are not apparently fitted for locomotion. In the males there is not so great a difference in character between the ambulacra of the trivium and those of the bivium; but the tentacles of the latter seem to be less fully developed in both sexes, and I have never happened to see an individual of either sex progressing upon, or adhering by, the water-feet of the dorsal canals.

"In a very large proportion of the females which I examined, young were closely packed in two continuous fringes adhering to the water-feet of the dorsal ambulacra. The young were in all the later stages of growth, and of all sizes from 5 up to 40 mm. in length; but all the young attached to one female appeared to be nearly of the same age and size. Some of the mothers with older families had a most grotesque appearance—their bodies entirely hidden by the couple of rows, of a dozen or so each, of yellow vesicles, like ripe yellow plums ranged along their backs, each surmounted by its expanded crown of oral tentacles; in the figure the young are represented about half-grown. All the young I examined were miniatures of their parents; the only marked difference was that in the young the ambulacra of the bivium were quite rudimentary—they were externally represented only by bands of a somewhat darker orange than the rest of the surface, and by lines of low papillæ in the young of larger growth; the radial vessels could be well seen through the transparent body-wall; the young attached themselves by the tentacular feet of the trivial ambulacra, which are early and fully developed.

"We were too late at the Falklands (January 23) to see the process of the attachment of the young in their nursery, even if we could have arranged to keep specimens alive under observation. There can be little doubt that, according to the analogy of the class, the eggs are impregnated either in the ovarial tube or immediately after their extrusion, that the first developmental stages are run through rapidly, and that the young are passed back from the ovarial opening, which is at the side of the mouth, along the dorsal ambulacra, and arranged in their places by the automatic action of the ambulacral tentacles themselves."

One other illustration we take, this time from an animal living in the surface water, though it sinks, when dead, to the bottom of the sea (Fig. 5).

"*Hastigerina murrayi* is very widely distributed on the surface of warm seas, more abundant, however, and of larger size in the Pacific than in the Atlantic. The shell consists of a series of eight or nine rapidly enlarging inflated chambers coiled symmetrically on a plane; the shell-wall is extremely thin, perfectly hyaline, and rather closely perforated with large and obvious pores. It is beset with a comparatively small number of very large and long spines. The proximal portion of each spine is formed of three laminæ, delicately serrated along their outer edges, and their inner edges united together. The spines, when they come near the point of junction with the shell, are contracted to a narrow cylindrical neck, which is attached to the shell by a slightly expanded conical base. The distal portion of the spine loses its three diverging laminæ, and becomes flexible and thread-like. The sarcode is of a rich orange colour from included highly-coloured oil globules.

"On one occasion in the Pacific, when Mr. Murray was out in a boat in a dead calm collecting surface creatures, he took gently up in a spoon a little globular gelatinous mass with a red centre, and transferred it to a tube. This globule gave us our first and last chance of seeing what a pelagic foraminifer really is when in its full beauty. When placed under the microscope it proved to be a *Hastigerina* in a condition wholly different from anything which we had yet seen. The spines, which were mostly unbroken, owing to its mode of capture, were enormously long, about fifteen times the diameter of the shell in length; the sarcode, loaded with its yellow oil-cells, was almost all outside the shell, and beyond the fringe of yellow sarcode the space between the spines, to a distance of about twice the diameter of the shell all round, was completely filled up with delicate *bullæ*, like those which we see in some of the Radiolarians, as if the most perfectly transparent portion of the sarcode had been blown out into a delicate froth of bubbles of uniform size. Along the spines fine double threads of transparent sarcode, loaded with minute granules, coursed up one side and down the other, while between the spines independent thread-like pseudopodia ran out, some of them perfectly free, and others anastomosing with one another or joining the sarcodic sheaths of the spines, but all showing the characteristic flowing movement of living protoplasm."

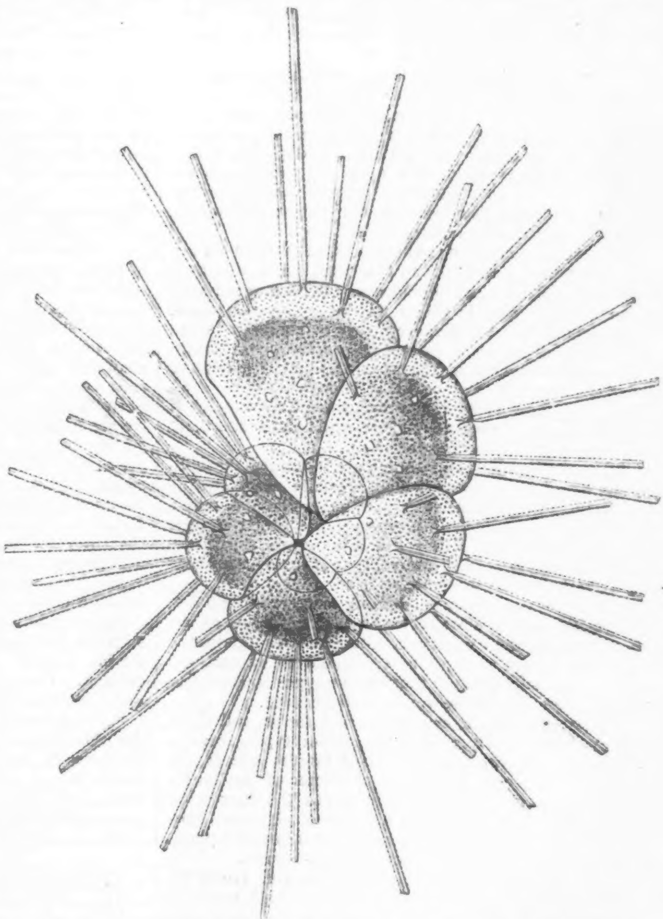


FIG. 5.—*Hastigerina murrayi*, Wyville Thomson. From the surface. Fifty times the natural size.

It would be easy to extend our notice on the animal forms alluded to, but our space forbids. It is curious that no vegetable life seems to have been met with in depths below 100 fathoms. "No plants live, so far as we know, at great depths in the sea; and it is in all probability essentially inconsistent with their nature and mode of nutrition that they should do so." But parasitic algæ have been detected in some of the deep-sea corals, and we are a little surprised to see the position of the diatoms queried; surely their plant affinities cannot now be discussed, and without these little plants we fancy some of the plant-eating deep-sea forms of animal life would be badly off. Holothuroids are especially fond of them.

The following general conclusions are arrived at:—

"1. Animal life is present on the bottom of the ocean at all depths.

"2. Animal life is not nearly so abundant at extreme as it is at more moderate depths; but, as well-developed members of all the marine invertebrate classes occur at all depths, this appears to depend more upon certain causes affecting the composition of the bottom deposits, and of the bottom water involving the supply of oxygen, and of carbonate of lime, phosphate of lime, and other materials necessary for their development, than upon any of the conditions immediately connected with depth.

"3. There is every reason to believe that the fauna of deep water is confined principally to two belts, one at and near the surface, and the other on and near the bottom; leaving an intermediate zone in which the larger animal forms, vertebrate and invertebrate, are nearly or entirely absent.

"4. Although all the principal marine invertebrate groups are represented in the abyssal fauna, the relative proportion in which they occur is peculiar. Thus Mollusca in all their classes, Brachyourous Crustacea, and Annelida, are on the whole scarce; while Echinodermata and Porifera greatly preponderate.

"5. Depths beyond 500 fathoms are inhabited throughout the world by a fauna which presents generally the same features throughout; deep-sea genera have usually a cosmopolitan extension, while species are either universally distributed, or, if they differ in remote localities, they are markedly representative, that is to say, they bear to one another a close genetic relation.

"6. The abyssal fauna is certainly more nearly related than the fauna of shallower water to the faunæ of the tertiary and secondary periods, although this relation is not so close as we were at first inclined to expect, and only a comparatively small number of types supposed to have become extinct have yet been discovered.

"7. The most characteristic abyssal forms, and those which are most nearly related to extinct types, seem to occur in greatest abundance and of largest size in the southern ocean; and the general character of the faunæ of the Atlantic and of the Pacific gives the impression that the migration of species has taken place in a northerly direction, that is to say, in a direction corresponding with the movement of the cold under-current.

"8. The general character of the abyssal fauna resembles most that of the shallower water of high northern and southern latitudes, no doubt because the conditions of temperature, on which the distribution of animals mainly depends, are nearly similar."

These volumes form a distinct contribution to Science, and will certainly be welcomed by the scientific worker; and their interest to the general reader, who can pass over the few technical descriptions of the new forms, will be scarcely at all less.

THE MODERN TELESCOPE[1]

III.

We know that both with object-glasses and reflectors a certain amount of light is lost by imperfect reflection in the one case, and by reflection from the surfaces and absorption in the other; and in reflectors we have generally two reflections instead of one. This loss is to the distinct disadvantage of the reflector, and it has been stated by authorities on the subject, that, light for light, if we use a reflector, we must make the aperture twice as large as that of a refractor in order to make up for the loss of light due to reflection. But Dr. Robinson thinks that this is an extreme estimate; and with reference to the four-foot reflector now in operation at Melbourne, and of which mention has already been made, he considers that a refractor of 33·73 inches aperture would be probably something like its equivalent if the glass were perfectly transparent, which is not the case.

On the assumption, therefore, that no light is lost in transmission through the object-glass, Dr. Robinson estimates that the apertures of a refractor and a reflector of the Newtonian construction must bear the relation to each other of 1 to 1·42. In small refractors the light absorbed by the glass is small, and therefore this ratio holds approximately good, but we see from the example just quoted how more nearly equal the ratio becomes on an increase of aperture, until at a certain limit the refractor, aperture for aperture, is surpassed by its rival, supposing Dr. Robinson's estimate to be correct. But with specula of silvered glass the reflective power is much higher than that of speculum metal; the silvered glass being estimated to reflect about 90 per cent.[1] of the incident light, while speculum metal is estimated to reflect about 63 per cent.; but be these figures correct or not, the silvered surface has undoubtedly the greater reflective power; and, according to Sir J. Herschel, a reflector of the Newtonian construction utilises about seven-eighths of the light that a refractor would do.

In treating of the question of the future of the telescope, we are liable to encroach on the domain of opinion, and go beyond the facts vouched for by evidence, but there are certain guiding principles which are well worthy of consideration. These have lately been discussed by Mr. Howard Grubb in a paper "On Great Telescopes of the Future." We shall take up his points *seriatim*, premising that in the two classes of telescopes, refractors and reflectors, each possesses some advantages over the other.

We may conveniently consider first the advantages which the refractor has over the reflector.

First, there is less loss of light with the former than with the latter, *as a rule*, hence for equal "space-penetrating power" the aperture of the reflector must be greater. This condition gives us a greater column of air and consequently greater atmospheric disturbance.

"The refractor having a tube closed at both ends, and the reflector being open at the upper end, the condition of air-currents is quite different in the two cases, to the disadvantage of the reflector, for in it the upper end being open, there is nothing to prevent currents of hot and cold air up and down the tube, and in and out of the aperture, and for this reason great advantage has been

[1] Continued from p. 127.

[1] Sir John Herschel, in his work on the telescope, gives the following table of reflective powers:—

After transmission through one surface of glass not in contact with any other surface	0·957
After transmission through one common surface of two glasses cemented together	1·000
After reflection on polished speculum metal at a perpendicular incidence	0·632
After reflection on polished speculum metal at 45° obliquity...	0·690
After reflection on pure polished silver at a perpendicular incidence	0·905
After reflection on pure polished silver at 45° obliquity ...	0·910
After reflection on glass (external) at a perpendicular incidence	0·043

The effective light in reflectors (irrespective of the eye-pieces) is as follows:—

Herschelian (Lord Rosse's speculum metal)	A. 0·632
Newtonian (both mirrors ditto)	B. 0·436
Do (small mirror or glass prism)...	C. 0·632
Gregorian or Cassegrain	D. 0·399
The same telescopes, all the metallic reflections being from pure silver	A. 0·905 B. 0·824 C. 0·905 D. 0·819

found in ventilating the tubes, *i.e.* making it of some open-work construction, in order that the air may pass through and across and remove currents of differing temperatures. This difficulty is not felt with refractors; but, curious to say, in the largest refractor at present in existence (the Washington 26-inch), Prof. Newcomb informs me that considerable inconvenience is felt sometimes from the outside of the object-glass cooling down more quickly in the evening than the inside, which produces a decided effect on the spherical aberration, and injures temporarily the otherwise fine definition. He consequently recommends the use of lattice or ventilated tubes for very large refractors. If this be found necessary, this advantage of the refractor vanishes."

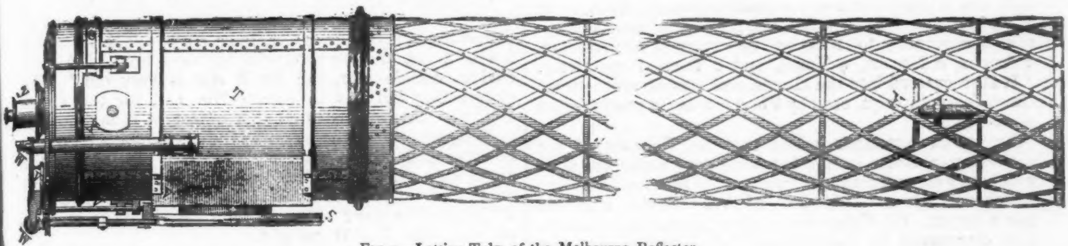


FIG. 9.—Lattice Tube of the Melbourne Reflector.

But there is another nice point concerning this larger aperture which has to be considered.

We may set out with observing that the light-grasping power of the reflector varies as the square of the aperture multiplied by a certain fraction representing the proportion of the amount of reflected light to that of the total incident rays. On the other hand the power of the refractor varies as the square of the aperture multiplied by a certain fraction representing the proportion of transmitted light to that of the total incident rays. Now in the case of the reflector the reflecting power of each unit of surface is constant whatever be the size of the mirror, but in that of the refractor *the transmitting power decreases with the thickness of the glass*, rendered requisite by increased size. Although for small apertures the transmitting power of the refractor is greater than the reflecting power of the reflector, still it is obvious that on increasing the size a stage must be at last reached when the two rivals become equal to each other. This limit has been estimated by Dr. Robinson to be 35·435 inches, a size not yet reached by our opticians by some ten inches, but object-glasses are increasing inch by inch, and it would be rash to say that this size cannot be reached within perhaps the lifetime of our present workers. However this may be we can say with safety that up to the present limit of size produced, refractors have the advantage in light-grasping power, and it is also a question whether with increase of thickness in the glass there will not be such an increase in the purity of material and polish as to keep the loss by transmission at its present value. Any one who has a Tully and a Cooke object-glass, by placing them side by side on a clean sheet of paper, will be able to see how our modern opticians have already reduced the loss by transmission.

The next point worthy of attention is the question of permanence of optical qualities. Here the refractor undoubtedly has the advantage. It is true that the flint glass of some object-glasses, chiefly those produced in Germany, gets attacked by a sort of tarnish, still that is not the case generally, while on the other hand, metallic mirrors often become considerably dimmed after a few months of use, the air of a town seeming to be fatal to them, and although repolishing is not a matter of any great difficulty in the hands of the maker, still it is a serious drawback to be obliged to return mirrors for this purpose. There are, however, some exceptions to this, for there are many small mirrors in existence whose polish is good after many years of continuous use, just as on the other hand there are many object-glasses whose polish has suffered in a few years, but these are exceptions to the rule. The same remarks apply to the silvered glass reflectors, for although the silvering of small mirrors is not a difficult process, the matter becomes exceedingly difficult with large surfaces, and indeed at present large discs of glass, say of four or six feet diameter, can rarely be produced. If, however, a process should be discovered of manufacturing these discs satisfactorily and of silvering them, there are objections to them on the grounds of the bad conductivity of glass, whereby changes of temperature alter the curvature, and there is also a great tendency for dew to be deposited on the surface.

With regard to the general suitability for observatory work this depends upon the kind of work required, whether for measuring positions, as in the case of the transit instrument, where permanency of mounting is of great importance, or for physical astronomy, when a steady image for a time only is required. For the first purpose the refractor has decidedly the advantage, as the object-glass can be fixed very nearly immovably in its cell, whereas its rival must of necessity, at least with present appliances, have a small, yet in comparison considerable, motion.

The difficulty of mounting mirrors, even of large size, has now been got over very perfectly. This difficulty does not occur in the mounting of object-glasses of sizes at present in use, but when we come to deal with lenses of some thirty inches diameter, the present simple method will in all probability be found insufficient, but we anticipate that one will be adopted which will allow the permanent position of the object-glass to be retained.

J. NORMAN LOCKYER

(*To be continued.*)

OUR ASTRONOMICAL COLUMN

THE COMET OF 1106.—In Mr. Williams's account of the object observed by the Chinese in this year, and called a comet by Ma Twan Lin, we find the following note:—"This appears to have been a large meteor, as it seems to have been seen for a short time only." It is probable that the author had not compared Pingré's description of the motion of the comet, which was certainly observed in Europe early in the year, or he would have seen that in all likelihood, notwithstanding Ma Twan Lin's account reads as if it referred to a temporary phenomenon, the Chinese really observed the bright comet recorded by the European historians. We are told that in the fifth year of the epoch Tsung Ning, on day Woo Seuh of the first moon (1106, Feb. 10) a

comet appeared in the west; it was like a great Pei Kow (a kind of measure). The luminous envelope was scattered; it appeared like a broken-up star. It was sixty cubits in length and three cubits in breadth. Its direction was to the north-east; it passed through Kwei, Lew, Wei, Maou, and Peih, which are sidereal divisions determined according to Biot by the stars β Andromedæ, β Arietis, α Muscæ, η Tauri, and ϵ Tauri respectively. "It then entered into the clouds, and was no more seen." Gaubil's manuscript, used by Pingré, assigns precisely the same course.

European historians relate that on February 4 (or according to others on the following day) a star was seen which was distant from the sun only "a foot and a half;" Matthew Paris and Matthew of Westminster call this star a comet. On February 7 a comet, properly so called, was discovered in Palestine in "that part of the sky where the sun sets in winter," its ray had "the whiteness of snow," and extended to the commencement of the sign Gemini, below the constellation Orion. As Pingré points out the comet must at this time have had a south latitude, and, considering the sun's position, could not be less advanced than 10° or 12° of Pisces to have been seen in the evening after sunset. The comet subsequently passed by west to north-west, the tail directed to that part of the sky between the north and the east; the comet was visible until the middle of the night, and "shone during twenty-five days in the same manner at the same hour;" as one writer states, it had a real motion from west to east. The length of the comet's appearance is variously given; an eye-witness says that the most piercing sight could hardly distinguish it after fifty days, and a manuscript consulted by Pingré, in the Bibliothèque de Sainte-Geneviève, of the thirteenth century at latest, mentions fifty-six days for the duration of visibility.

The comet of 1106 long attracted attention from the circumstance of Halley having identified it as the famous comet of 1680, an idea which was first disputed by Dunthorne, on the authority of a manuscript preserved in one of the College libraries at Cambridge, which gives the comet's track from the beginning of the sign Pisces (on February 7 as Dunthorne reads) in the order of the signs to the commencement of Cancer, which agrees closely with the path recorded by the Chinese. He considered that this track "quite overbalanced the probability of the identity of the comet with that of 1680"—and this view has been confirmed by subsequent calculation. Again, when astronomers were searching for earlier accounts which might refer to the great comet of 1843, first detected at noon-day on the date of its perihelion passage, this comet of 1106 was fixed upon by MM. Laugier and Mauvais, as probably identical with it, several of the circumstances mentioned above being overlooked by them, particularly the fact of the comet having been observed so long in the northern part of the heavens, where it is impossible that the comet of 1843 could be located.

On carefully weighing the scanty evidence afforded by the records of the time, it appears likely that the elements of the comet of 1106 bore some resemblance to those of the great comet of 1618 (Pingré's third comet), the inclination being smaller.

THE SATELLITES OF MARS.—Both of the newly-discovered satellites of Mars were observed during September with the 12-inch equatorial of the Morrison Observatory, Glasgow, Missouri, by Mr. Pritchett. On September 7 the two satellites could be seen with the planet entirely in the field, and were very distinct when it was shut out of it, and on September 10 and 13, the inner one was easily observed. The outer satellite was again estimated to be of the fourteenth magnitude. The observations of this satellite were made with wires faintly illuminated with a red light; for observations of the inner one the light of the planet sufficed. Unfavourable skies prevented any observations in October, though Mr. Pritchett thinks the satellites might have been well followed during that month.

COLOURED DOUBLE STARS.—In Sir John Herschel's seventh catalogue of double stars from the sweeps with the 20-feet reflector is one the position of which identifies it with Σ 724, and the note attached runs thus: "A very curious double star, the small star is very red." The observation belongs to sweep No. 121, for the epoch 1828·05. Struve measured this object in 1829, but says nothing respecting the colours of the components, which he estimated on his scale 8·7 and 10·0. In 1829·85 the angle was 241·5°, and the distance 6·86″. Has any one confirmed Sir John Herschel's observation on the colour of the smaller star? The position for 1878·0 is in R.A. 5h. 33m. 30s., N.P.D. 79° 5′·5.

In *Memorie del' Osservatorio del Collegio Romano*, 1857-59, p, 173, Secchi mentions a wide double star, which is called *nova*, and is thus measured :—

1856·63 Pos. 335°·25 Dist. 23″·83 { Components 7m. and 8m. A red, B blue.

He has the additional remark, "*Colori superbi.*" This object would appear to be formed by Nos. 3743 and 3744 of Zone + 37° of the *Durchmusterung*; positions for 1855·0 :—

		h. m. s.		° ′
3743	R.A.	19 58 23·5	N.P.D.	52 4·1
3744	"	19 58 25·5	"	52 4·7

THE TALKING PHONOGRAPH [1]

MR. THOMAS A. EDISON recently came into this office, placed a little machine on our desk, turned a crank, and the machine inquired as to our health, asked how we liked the phonograph, informed us that *it* was well, and bid us a cordial good night. These remarks were not only perfectly audible to ourselves, but to a dozen or more persons gathered around, and they were produced by the aid of no other mechanism than the simple little contrivance explained and illustrated below.

The principle on which the machine operates we recently explained quite fully in announcing the discovery. There is, first, a mouth-piece, A, Fig. 1, across the inner orifice of which is a metal diaphragm, and to the centre of this diaphragm is attached a point, also of metal. B is a

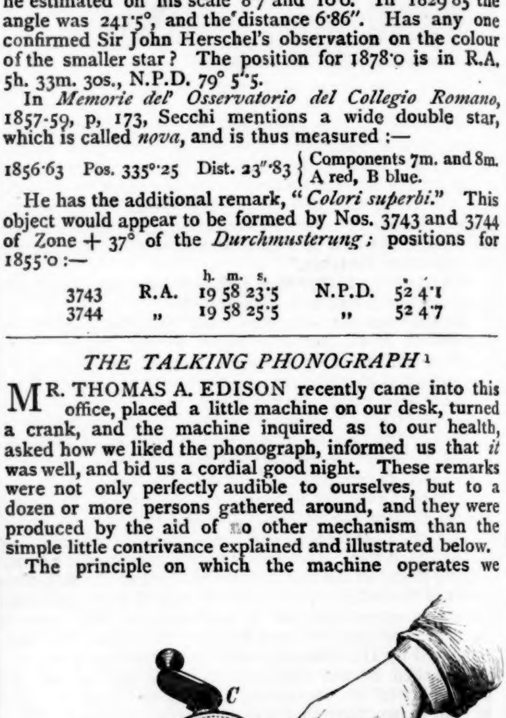


FIG. 1.

[1] From the *Scientific American* of December 22, 1877.

brass cylinder supported on a shaft which is screw-threaded, and turns in a nut for a bearing, so that when the cylinder is caused to revolve by the crank, C, it also has a horizontal travel in front of the mouthpiece, A. It will be clear that the point on the metal diaphragm must, therefore, describe a spiral trace over the surface of the cylinder. On the latter is cut a spiral groove of like pitch to that on the shaft, and around the cylinder is attached a strip of tinfoil. When sounds are uttered in the mouth-piece, A, the diaphragm is caused to vibrate, and the point thereon is caused to make contacts with the tinfoil at the portion where the latter crosses the spiral groove. Hence, the foil, not being there backed by the solid metal of the cylinder, becomes indented, and these indentations are necessarily an exact record of the sounds which produced them.

It might be said that at this point the machine has already become a complete phonograph or sound writer, but it yet remains to translate the remarks made. It should be remembered that the Marey and Rosapelly, the Scott or the Barlow apparatus, which we recently described, proceed no further than this. Each has its own system of caligraphy, and after it has inscribed its peculiar sinuous lines, it is still necessary to decipher them. Perhaps the best device of this kind ever contrived was the preparation of the human ear made by Dr. Clarence J. Blake, of Boston, for Prof. Bell, the inventor of the telephone. This was simply the ear from an actual subject, suitably mounted, and having attached to its drum a straw, which made traces on a blackened rotating cylinder. The difference in the traces of the sounds uttered in the

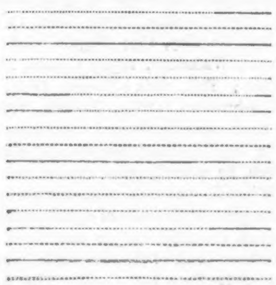


Fig. 2.

ear was very clearly shown. Now there is no doubt that by practice and the aid of a magnifier, it would be possible to read phonetically Mr. Edison's record of dots and dashes, but he saves us that trouble by literally making it read itself. The distinction is the same as if, instead of perusing a book ourselves we drop it into a machine, set the latter in motion, and, behold! the voice of the author is heard repeating his own composition.

The reading mechanism is nothing but another diaphragm held in the tube D on the opposite side of the machine, and a point of metal which is held against the tin foil on the cylinder by a delicate spring. It makes no difference as to the vibrations produced, whether a nail moves over a file or a file moves over a nail, and in the present instance it is the file or indented foil strip which moves, and the metal point is caused to vibrate as it is affected by the passage of the indentations. The vibrations, however, of this point must be precisely the same as those of the other point which made the indentations, and these vibrations, transmitted to a second membrane, must cause the latter to vibrate similar to the first membrane, and the result is a synthesis of the sounds which, in the beginning, we saw, as it were, analysed.

In order to exhibit to the reader the writing of the machine which is thus automatically read, we have had a cast of a portion of the indented foil made, and from this the dots and lines in Fig. 2 are printed in, of course absolute facsimile, excepting that they are level instea of being raised above or sunk beneath the surface. This is a part of the sentences, "How do you do?" and "How do you like the phonograph?" It is a little curious that the machine pronounces its own name with especial clearness. The crank handle shown in our perspective illustration of the device does not rightly belong to it, and was attached by Mr. Edison in order to facilitate its exhibition to us.

In order that the machine may be able exactly to reproduce given sounds, it is necessary, first, that these sounds should be analysed into vibrations, and these registered accurately in the manner described; and second, that their reproduction should be accomplished in the same period of time in which they were made, for evidently this element of time is an important factor in the quality and nature of the tones. A sound which is composed of a certain number of vibrations per second is an octave above a sound which registers only half that number of vibrations in the same period. Consequently if the cylinder be rotated at a given speed while registering certain tones, it is necessary that it should be turned at precisely that same speed while reproducing them, else the tones will be expressed in entirely different notes of the scale, higher or lower than the normal note as the cylinder is turned faster or slower. To attain this result there must be a way of driving the cylinder, while delivering the sound or speaking, at exactly the same rate as it ran while the sounds were being recorded, and this is perhaps best done by well-regulated clockwork. It should be understood that the machine illustrated is but an experimental form, and combines in itself two separate devices—the phonograph or recording apparatus, which produces the indented slip, and the receiving or talking contrivance which reads it. Thus in use the first machine would produce a slip, and this would for example be sent by mail elsewhere, together in all cases with information of the velocity of rotation of the cylinder. The recipient would then set the cylinder of his reading apparatus to rotate at precisely the same speed, and in this way he would hear the tones as they were uttered. Differences in velocity of rotation within moderate limits would by no means render the machine's talking indistinguishable, but it would have the curious effect of possibly converting the high voice of a child into the deep bass of a man, or *vice versâ*.

No matter how familiar a person may be with modern machinery and its wonderful performances, or how clear in his mind the principle underlying this strange device may be, it is impossible to listen to the mechanical speech without his experiencing the idea that his senses are deceiving him. We have heard other talking machines. The Faber apparatus, for example, is a large affair, as big as a parlour organ. It has a key-board, rubber larynx and lips, and an immense amount of ingenious mechanism which combines to produce something like articulation in a single monotonous organ-note. But here is a little affair of a few pieces of metal, set up roughly on an iron stand about a foot square, that talks in such a way, that, even if in its present imperfect form many words are not clearly distinguishable, there can be no doubt but that the inflections are those of nothing else than the human voice.

We have already pointed out the startling possibility of the voices of the dead being reheard through this device, and there is no doubt but that its capabilities are fully equal to other results just as astonishing. When it becomes possible, as it doubtless will, to magnify the sound, the voices of such singers as Parepa and Titiens will not die with them, but will remain as long as the metal in which they may be embodied will last. The witness in court will find his own testimony repeated by machine, confronting him on cross-examination—the testator will repeat his last will and testament into the machine so

that it will be reproduced in a way that will leave no question as to his devising capacity or sanity. It is already possible by ingenious optical contrivances to throw stereoscopic photographs of people on screens in full view of an audience. Add the talking phonograph to counterfeit their voices, and it would be difficult to carry the illusion of real presence much further.

NOTES

MR. SORBY is busy perfecting his new method of studying minerals. Some very remarkable properties are still unexplained, and only the other day Mr. Sorby made a very great fresh advance in the subject.

IT is probable that Sheffield will be chosen for the meeting of the British Association in 1879. Nottingham was to have been the place of meeting, but a difficulty has arisen respecting the meeting there, and Sheffield has been unofficially written to. The matter is being warmly taken up by some of the principal townsmen, and there can be no doubt with a successful result.

A PHOTOLITHOGRAPHIC plate of the primary triangulation of the United States Geological and Geographical Survey of the Territories, carried on during the summer of 1877, by Mr. A. D. Wilson, chief topographer, has just been published by the United States Geological Survey, under the charge of Dr. F. V. Hayden. The area covered by these triangles extends from Fort Steele, in Wyoming Ty., westward to Ogden, in Utah Ty., a distance of about 260 miles, and north as far as the Grand Teton, near the Yellowstone National Park, including Freemont's Peak of the Wind River Range of the Rocky Mountains. The area embraces about 28,000 square miles, and within it, twenty-six primary stations were occupied, and their positions accurately computed. Besides these occupied stations, a large number of mountain peaks were located, which in the future will be occupied as points for the extension of the topographical work of the Survey. A base line was carefully measured near Rawlin's Springs, on the line of the Union Pacific Railroad, and from this initial base the work was extended north and west to the valley of Bear River, in Idaho Ty. Here a check base was measured, and the system expanded to the neighbouring mountain peaks to connect with the triangulation as brought forward from the first-mentioned base. Along the line of the Union Pacific Railroad the work was connected at six points with the triangulation system of Clarence King's 40th parallel survey. In addition to the importance of this sheet as the base work of the season's topographical work, it presents a most striking feature in the number of remarkably long sights which were taken from the summits of some of the most lofty mountains in the area explored. Many of these sights were over 100 miles in length, while some reach a distance of 135 miles. From Wind River Peak all the prominent points in the Big Horn Mountains were sighted, also the loftier peaks of the Uinta Mountains; the former are located 165 miles to the north-east, while the Uinta Mountains are situated about the same distance to the south-west. As these ranges were not in the scope of the season's work, they are not given on the chart.

THE *Annual Report of the Smithsonian Institution* for the year 1876, which has recently been published, is of general interest. The Institution continues to carry on its usual work with vigour and efficiency. Two important volumes of the Smithsonian Contributions to Knowledge, xx. and xxi., have been issued. The former on the Winds of the Globe, by Prof. Coffin, consisting of 781 quarto pages, is considered to be the most important contribution to knowledge which the Institution has given to the world. It presents a rich mine of information for the use of meteorologists, the physical geographer, and the mariner. Volume xxi. contains the following articles, viz.:—(1) Statements and Expositions of Certain Harmonies of the Solar System, by Prof. Alexander. (2) On the General Integral of Planetary Motion, by Prof. Newcomb. (3) The Haidah Indians by J. G. Swan. (4) Tables of Atmospheric Temperature in America. There has been published an important work on the Antiquities of Tennessee, by Dr. Joseph Jones, and another on the Archæological Collections of the U.S. National Museum; also a supplement to Prof. F. W. Clarke's work on the "Constants of Nature," consisting of tables of specific gravities, boiling and melting points, specific heats, &c. Large additions have during the year been made to the collections of the National Museum in charge of the Institution. In the Appendix to the Report there is a translation of the eulogy on Gay-Lussac by M. Arago; a biographical sketch of Dom Pedro II.; a translation of an important paper of Prof. Pilar on the Revolutions of the Earth's Crust, which will be read with interest by students of physical geography and geology. The subjects discussed in this article are the origin of the earth, central heat, the fluid envelope, organisms, ice, with a concise account of the theory of secular changes of climate resulting from changes in the eccentricity of the earth's orbit, antiquity of man, &c. Then follows a paper by Dr. D. Kirkwood on the Asteroids between Mars and Jupiter. But the article which will probably attract most attention is one by Mr. W. B. Taylor on Kinetic Theories of Gravitation. In this memoir, occupying about eighty pages, is given an interesting historical account of all the principal theories which have been advanced since the time of Newton to the present day to explain the nature of gravitation. Villemot, 1707; Bernouilli, 1734; Le Sage, 1750; Euler, 1760; Herapath, 1816; Guyot, 1832; Faraday, 1844; Seguin, 1848; Boucheporn, 1849; Lamè, 1852; Waterston, 1858; Challis, 1859; Glennie, 1861; Keller, 1863; Tait, 1864; Saigey, 1866; Croll, 1867; Leray, 1869; Boisbaudran, 1869; Guthrie, 1870; Crookes, 1873. These theories are all criticised with considerable acuteness. Mr. Taylor lays down six fundamental characteristics of gravity with which, he asserts, every theory must agree. But unfortunately it is in reference to the truth of some of Mr. Taylor's postulates that the greatest diversity of opinion exists. No kinetic theory of gravitation can fulfil his six conditions. Mr. Taylor seems to misapprehend some of the theories in important points, particularly those of Le Sage and Croll. The Appendix concludes with a number of interesting papers on Ethnology.

WE have already referred to Prof. A. Agassiz's intention of carrying out a series of researches in the Gulf of Mexico. With an assistant he is to be accommodated on board the United States Coast Survey steamer *Blake*, which has just sailed on a surveying cruise that will occupy this winter in the Gulf of Mexico. By a study of the animals dredged from the bottom of the Gulf, Prof. Agassiz will be enabled to make important comparisons with the fauna of the Atlantic, and especially as to growth, habits, migrations, and changes of living forms found in the waters near the British Islands and the Scandinavian Peninsula. The expedition is under the command of Lieutenant-Commander Charles D. Sigsbee, United States Navy, who has had several years experience on coast survey duty, and has been notably successful in deep-sea soundings.

NEW YORK will in all probability have a magnificent new Zoological Garden in Central Park before the end of another year. The Park Commissioners have little doubt that the amount of money, 300,000 dollars, necessary to make a commencement, will be subscribed without difficulty.

THE death is announced of Mr. Robert Hollond, a gentleman formerly well-known in connection with aeronautics.

THE Rev. Horace Waller writes to the *Times* that Col. Mason has been round Lake Albert Nyanza in a steamer, and

corroborates the fact of its being a comparatively small land-locked lake. Col. Mason is in the service of the Khedive.

MR. STANLEY has arrived in Egypt, and is to spend a few days at Cairo. On New Year's Day he was to be entertained at a banquet by Sir George Elliot, M.P., to which the principal English and American visitors and residents were invited.

THE African Association presided over by the King of the Belgians has learnt by telegram that its travellers have safely reached Zanzibar.

AT a recent meeting of the Liverpool Historic Society, Mr. T. Glazebrook Rylands, F.S.A., read an important paper on "Ptolemy's Geography of the Coast from Caernarvon to Cumberland (including Cheshire and Lancashire). The paper was the preliminary result of extensive and long research, during which the author has found out that previous writers have examined Ptolemy's work carelessly or inadequately, and greatly misrepresented his *data*. It has, for example, been inferred for centuries that the Mersey was unknown to Ptolemy, and that the river known as Belisama was identical with the Ribble. This has led to further deductions of an erroneous character; as, for example, that there was a wide sheet of water making the mouths of the Mersey and the Dee undistinguishable, while two islands in it reared their heads, viz., Wallasey, separated by a branch of the tide through Wallasey Pool, and Wirral, separated by a strait almost coincident with the canal from Chester. Mr. Rylands believes—and there can be little doubt of the fact—that he has ascertained the ideas of Ptolemy and verified his measurements and mode of projection in a way wholly unknown to former inquirers. He has thus explained apparent anomalies and corrected misunderstandings of former writers. Commencing southwards at Caernarvonshire, he has verified the positions from beyond Pwlhelli round by Caernarvon and Conway to the Dee; he has verified the positions of the Mersey and the Ribble, and all along the coast to St. Bee's Head, in Cumberland. In several instances where it was thought Ptolemy was in error, Mr. Rylands has shown he is correct, and it is a matter of surprise that where we should expect approximate truth only, the more rigid tests give more accurate results.

THE Bristol Naturalists' Society appears to be in a flourishing condition. It has recently added to its organisation a Physical and Chemical "Section," of which Dr. W. A. Tilden is secretary and Mr. P. J. Worsley president. The recent meetings of the Society have been more largely attended, and there appears to be a revived interest in physical science in the ancient city.

M. GAUTHIER VILLARS has just published a new edition of a highly interesting old book, "Lectures on Chemical Philosophy," delivered at the Collége de France in 1836 by M. Dumas. In this curious work all the prevalent ideas in chemistry were initiated. Not a single sentence has been altered, yet M. Dumas' lectures seem quite fresh and young, ready to be used by students in the highest schools. They were collected by M. Bineau, a gentleman who died twenty years ago, after having been a professor in the Lyons Faculty of Sciences.

THE sittings of the enlarged council of the Paris observatory came to an end last week. The resolutions come to, of which we have already given the substance, have been sent to M. Bardoux, the Minister for Public Instruction. The International Meteorological Service entered, on January 1, the twenty-first year of its existence, and will continue connected with the Paris Observatory, where it was established by M. Leverrier in 1857. The present head of the service is M. Front, a physicist connected with the service for many years, and trained by Leverrier himself. The first physicist-*adjoint* is M. Moureau, formerly a schoolmaster, whom Leverrier remarked for his zeal, and assiduity in meteorological researches and observations. The great astronomer required no other scientific qualifications than intelligence and instruction obtained by personal exertion. He turned away many doctors in science and pupils of the highest schools who were wanting in the requirements he was anxious to secure, and sought to find them even in the humblest stations of life.

MR. JOHN FIELDING, of Todmorden, has just presented to the Aquarium at Westminster two specimens of *Proteus sanguineus*, obtained by his courier, C. F. Kohl, from the grotto of Adelsburg. They are said to be the first shown in England. A specimen of *Menobranchus lateralis* has been on view for some little time.

A MOST unfortunate series of disasters followed Mr. Carrington's endeavours to bring to London a collection of specimens of the Mediterranean fauna. Dr. Eisig, of the Naples Aquarium, offered him every facility, suggested localities, and placed some store-tanks at his service. A collection of fish zoophytes and corals was made, and seven tanks were fitted up on a cargo steamer to transport them to England. Shortly after starting a thunderstorm was encountered, the ship was struck by lightning, and the contents of two metal tanks were at once destroyed. Among other things a fine collection of mureen eels averaging 2 feet in length was thus lost. Rough weather for a day or two caused further deaths. After leaving Gibraltar the change of temperature proved excessively fatal, the bright-coloured animals suffering most. The heavy weather in the Channel broke some of the other tanks, so that but few animals reached England alive. Mr. Carrington, however, arranged for supplies with agents at Naples, Messina, Palermo, Valentia, Gibraltar, Tangiers, and Lisbon.

MR. B. RALPH, of Launceston, Cornwall, sends us a ripe strawberry which he gathered on December 29 from a hedge about 400 feet above sea-level. Pink strawberry blossoms, he states, are not uncommon. The thermometer stood at 50° in the shade. Bar. 29·2, with a west wind. Many of the commoner hedgeflowers linger on, such as lychnis and geranium. He also incloses some primroses, blossoming in an exposed situation outside his window.

Two somewhat forcible shocks of earthquake were felt at Bologna on December 23, and a slight one at Alicante on the preceding day.

PROF. BARRETT, in a recent lecture on the telephone, gave a receipt for making a cheap one. Take a wooden tooth-powder box and make a hole about the size of a half-crown in the lid and the bottom. Take a disc of tinned iron, such as can be had from a preserved meat tin, and place it on the outside of the bottom of the box, and fix the cover on the other side of it. Then take a small bar-magnet, place on one end a small cotton or silk reel, and round the reel wind some iron wire, leaving the ends loose. Fix one end of the magnet near, as near as possible without touching, to the disc, and then one part of the telephone is complete. A similar arrangement is needed for the other end. The two are connected by the wire, and with this Prof. Barrett says he has been able to converse at a distance of about 100 yards.

M. BARDOUX, the new Minister of Public Instruction in France, has held a reception of the several heads of his department and *employés* of the central administration. He delivered a speech insisting upon the necessity for a Republican Government to educate the people, as a good system of public education is the strongest basis on which any Republic can be safely established. According to the *XIX. Siècle* M. Bardoux is not only preparing a Bill for establishing gratuitous elementary education, but also for organising a higher elementary education.

PROF. PFAUNDLER communicated in a recent session of the Vienna Academy the results of some experiments undertaken to decide the question as to the smallest absolute number of vibrations capable of producing a sound. By means of a siren with two openings for blowing, he finds that two isolated vibrations are capable of producing a tone which, by repetition, becomes audible.

THE Meteorological Society of Paris has elected as president M. Hervé Mangon, professor of Agriculture at the Conservatoire des Arts et Métiers.

THE Postal and Telegraph services are to be united in France, as they have been already in England, under a single direction. The first director of the complex organisation will be M. Cocheris, one of the staff of the *Temps* and a well-known writer on matters of political economy.

AT the last meeting, December 19, 1877, of the Russian Geographical Society, M. Mushketov made a very interesting communication on his last journey in the Tian Shan and to the Pamir, where he visited some places never before visited by European travellers. His researches enable us to correct many imperfections in the works of Gordon and Stoliczka, and to obtain many new and important data. A complete geological sketch of the Pamir highlands will soon be published by M. Mushketov. At the same meeting the secretary gave an account of a new expedition to Central Asia, which will start from St. Petersburg at the beginning of this year, under the leadership of Prof. A. E. Middendorff. The expedition has especially in view the study of the agricultural conditions of Turkistan, and the well-known traveller, zoologist, and practical agriculturist who is at the head of the expedition, will be supported in his work by MM. Smirnoff and Russow.

AT the meeting, December 15, of the St. Petersburg Society of Naturalists Prof. Kessler referred to the fishes brought this year by M. Polyakoff from the lakes Ala-Kul and Balkhash. In addition to the seven species which were known before in the Central-Asian fauna he has discovered four new ones, one of which is the interesting fish described by the inhabitants as *Marenker* (its zoological description will soon appear), the flesh and caviare of which are poisonous.

PROF. BERTHELOT, of Paris, is probably the most prolific chemist of the day. We notice in the two last numbers of the *Annales de Chimie et de Physique*, the two last numbers of the *Comptes Rendus*, and the last *Bulletin de la Société Chimique de Paris*, thirty-two various articles under his name. Berthelot's researches are, however, confined to thermal and physical chemistry, and are not delayed by the analytical operations attendant on other branches of chemical investigation.

CAPT. J. O. LUNGINERS, of the Danish vessel *Lutterfeld*, communicates to a Copenhagen paper an interesting account of a novel experience which occurred on December 10, 1876, while on a voyage to Valparaiso. The vessel was at this time in the neighbourhood of Terra del Fuego, about 140 miles from Magellan's Straits, when early in the morning it narrowly escaped collision with an island where no trace of land appeared on the charts. The vessel hove-to until daylight, when the captain proceeded with a boat's crew to the new island, which had gradually diminished in size since the first observation. Around the conical rocky mass the water was hissing, and although no smoke appeared, it was found to be too highly heated to permit of landing. The sinking continued slowly, until at eight o'clock the island was completely submerged, and an hour later the vessel passed over the spot where it had disappeared.

THE December Session of the Berlin Geographical Society was occupied by a long and interesting address from Dr. F. M. Hildebrandt, on the results of his late African explorations. We have already alluded in a late number to the unfortunate result of the expedition to the snow-clad mountains of equatorial Africa, when the explorer was compelled to return with Mount Kenia fairly in sight. The heroism of Dr. Hildebrandt in battling with danger and disease in manifold forms is only approached by the adroitness and ingenuity which characterised his dealings with the natives. Among the Hataitas he was regarded as a magician, and was forced to pronounce incantations on the unfruitful fields. For this purpose, at his request, specimens of all the plants and animals in the vicinity were gathered by the tribe, and after having served as a "fetish," were carefully packed away in the collections. On another occasion he was attacked by several hundred natives, who beat a hasty retreat, when the explorer advanced towards them armed with a photographic camera. Despite the constant succession of misfortunes accompanying Dr. Hildebrandt during his two years' explorations in Africa, he has succeeded in gathering together a large and valuable collection of anthropological and botanical specimens especially, from Cape Gardafui and the Comoro island Johanna. A number of new species and genera, particularly of aromatic plants, were discovered in the former locality.

THE additions to the Zoological Society's Gardens during the past week include two Lions (*Felis leo*) from Upper Nubia, presented by Mr. John Baird; a Green Monkey (*Cercopithecus callitrichus*) from West Africa, presented by Mr. J. Scott; a Bonnet Monkey (*Macacus radiatus*) from India, presented by Mr. J. H. Thompson; a Common Thicknee (*Œdicnemus crepitans*), European, presented by Mr. F. Möll; a Macaque Monkey (*Macacus cynomolgus*) from India, deposited; a Collared Fruit Bat (*Cynonycteris collaris*), a Geoffroy's Dove (*Peristera geoffroii*), bred in the Gardens.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—An examination for a Radcliffe Travelling Fellowship will be held on February 11. Candidates should forward notice of their intention to offer themselves, on or before January 15, to Dr. Acland.

An examination will be held at Queen's College on April 30 to fill up an open scholarship in natural science, of the value of 90*l.*, tenable for five years.

LONDON.—We learn that in consequence of the success attending the course of Lectures on Physiology now being delivered at the Working Men's College, Great Ormond Street, by Mr. T. Dunman, the Council of that Institution have arranged for the delivery, by the same gentleman, of a supplementary advanced course of about sixteen lectures, the first of which will be delivered on Friday, January 18. The lectures will be accompanied by practical demonstrations. Mr. Dunman has been appointed to the lectureship in physiology at the Birkbeck Institution, recently vacated by Dr. Aveling.

MANCHESTER.—Mr. M. M. Pattison Muir, F.R.S.E., Assistant-Lecturer in Chemistry, and Demonstrator in the Laboratory of the Owens College, has been appointed Prælector in Chemistry at Gonville and Caius College, Cambridge.

BERLIN.—We notice in the report of a late session of the Prussian House of Deputies a very vigorous presentation, by Prof. Mommsen and Prof. Virchow, of the necessity for a new building for the royal library. This valuable collection of books—over 700,000 in number—is the largest in Germany, and increases so rapidly that the present quarters in the Imperial Palace are most inadequate. The Government shows an inclination to remedy the evil, and it is to be hoped that this chief store of mental pabulum for the Berlin student will soon be provided with a house of its own, and the much-needed catalogue of its treasures finally be published.

BUDAPESTH.—The committee intrusted with the preparations for the first centennial celebration of this university, have decided to invite all foreign universities to send representatives on the occasion. A work on the history of the university is being prepared by Prof. Theodor Pauler, the late Minister of Education.

HEIDELBERG.—In the recently-issued calendar of the University for the present semester we notice a serious falling off in the attendance, the present number of students (461) being 250 less than that for the past summer. This fact is chiefly due to the increasing custom of the German students to gather in the Universities of the great cities during the winter. The theological faculty includes 19 students, the medical, 79, the philosophical, 180, and the legal, 183. Heidelberg still possesses evidently its traditional attractions for English-speaking students, the catalogue containing the names of twenty-one Englishmen and twenty-six Americans, a large proportion of whom are studying under Bunsen. Of the sixty-seven other foreigners in attendance Switzerland contributes eighteen and Russia nineteen. The corps of professors numbers 105, of whom fifty-nine are in the philosophical faculty. Prof. Bluntschli, the leading authority on international law, is the pro-rector for the present year. We notice that Prof. Blum has been forced by advanced age to give up the chair of mineralogy. His connection with the University dates back to 1828, and under his direction the mineralogical department at Heidelberg has long been one of the favourite resorts of students from various countries, the museum ranking among the best in Europe. Prof. Blum's fame as a mineralogist rests chiefly on his thorough and exhaustive researches on pseudomorphs, the results of which are embodied in his work "Die Pseudomorphosen des Mineralreiches." A very complete and practical text-book of mineralogy, as well as the numerous smaller treatises on subdivisions of the science, which have appeared at intervals from his pen, are regarded as standard works.

STRASSBURG.—The grant of money for the new edifices of the university amounts to 10,500,000 marks (over 500,000*l.*). Of this sum 2½ millions are contributed from the imperial funds, 5½ millions result from Alsace-Lorraine's share of the new imperial bank notes, and the remainder is contributed by the city, the district, and the two provinces. At present the university is attended by 627 students divided as follows among the faculties;—theological, 49, legal, 156, medical, 117, philosophical, 305. Despite the able corps of professors gathered together since the re-establishment of this historic university, the number of students shows a decrease of eighty as compared with 1876, a result due in a great measure to the coldness exhibited by the old French inhabitants towards the German students.

HOLLAND.—The Netherlands School Museum, at Amsterdam, was opened on December 24, 1877, in presence of Mr. Heemskerk (recently Prime Minister of Holland), and several authorities connected with the Educational Department. Mr. A. van Otterloo, for the committee, in his opening speech alluded to the valuable co-operation of England in the exhibition. The authorities afterwards inspected the museum, and expressed their high appreciation of the interesting collections of school appliances exhibited by the School Board for London and others.

ST. PETERSBURG.—A new High School for ladies is to be opened at St. Petersburg for the special purpose of preparing female teachers for women's colleges. The School is provided with the necessary money by a young lady, and it will be conducted by the professors of the St. Petersburg University.

Prof. Tarkhanoff, of the St. Petersburg Medical Academy, having assisted at the examinations in physiology and anatomy of the thirty-six ladies who have now finished their five years' course at the High School of Medicine at St. Petersburg, publishes a report on those examinations. The answers of the ladies, he says, were definite, clear, and often vivid. Deep and very accurate knowledge was shown in anatomy and histology, the examinations having been made according to the extensive programmes existing in ordinary universities. On the average the answers were quite as good as those of male students; but the answers of three or four ladies, by their completeness and brilliancy, produced a deep impression on the examiners, and greatly exceeded all the professor has ever witnessed either as a student or professor.

CHARKOW.—The annual calendar of this Russian university shows an attendance of 442 students. Over half of this number are freed entirely from the payment of lectures, while a third receive annual stipends varying from 180 to 340 roubles. The corps of instructors numbers sixty-four.

SOCIETIES AND ACADEMIES

LONDON

Royal Astronomical Society, December 14, 1877.—Dr. Huggins, F.R.S., in the chair.—A paper by Dr. Wolf, of Zurich, set forth that the sun-spot period varies from seven to sixteen years, eleven years being the average.—A paper by Mr. C. V. Boys described a new astronomical clock. Mr. Christie and Lord Lindsay criticised it.—A photograph of the sun was presented by M. Janssen. It is one of those taken daily at Meudon, measuring one foot in diameter. Dr. De la Rue said it was the finest example of celestial photography he had ever seen. It was not taken with an equatorial, but an instrument after the fashion of the Kew photoheliograph with a 5½-inch object-glass. The picture was not taken at the principal focus, but in that of a secondary magnifier, corrected independently of visual focus. He pointed out the tornadoes visible on the photograph, and spoke of the importance of a physical observatory to register the changes which occur on a tremendous scale every hour, sun-spots being phenomena of comparatively small importance. Capt. Abney spoke in corroboration, and said that M. Janssen at first thought these photographed tornadoes had an atmospheric origin. Mr. Christie said that similar phenomena had been found on the Greenwich photographs, and they had nothing to do with the collodion.—Mr. Glaisher read a paper on the law of force tending to any point whatever in the plane of motion in order that the orbit may always be a conic.—Mr. Lynn gave a description of Mr. Howlett's drawing of the solar spot of October 31 to November 3, being about 15″ diameter.—Lord Lindsay concluded the description of his spectroscope for nebulæ referred to last month.—Mr. Christie made some remarks and criticised it, and the meeting then adjourned.

Photographic Society, December 11, 1877.—Papers were read by Capt. Abney, R.E., F.R.S., on fog-producing emulsions and their rectification, and by H. B. Berkeley, on emulsions. Capt. Abney showed that the cure, or rather the elimination, of fog in emulsions (as also in dry plates) would be effected by the introduction of either bromide, iodine, or nitric acid into the emulsion. Nitric acid prevented the formation of any chemically-produced sub-bromide of silver, and reduced the fog to the state of bromide; where pure bromide is present, it seems almost impossible that there should be fog. If an emulsion plate is exposed to light, and afterwards partly dipped into copper bromide, then exposed in the camera and developed, the portion treated with the bromide will be found to be free from fog and perfectly clear.

PARIS

Academy of Sciences, December 17, 1877.—M. Peligot in the chair:—The following papers were read:—On the order of appearance of the first vessels in the shoots of some Leguminosæ (third part), by M. Trécul.—Note on the ring of Saturn, by M. Tisserand.—On intramolecular work, by M. Boileau.—On an essential improvement of the navigation lock with mixed oscillation, by M. De Caligny.—M. Cailletet was elected correspondent for the section of mineralogy, in room of the late M. d'Ommalius d'Alloy (obtaining thirty-three votes against nineteen for Mr. James Hales).—Production of crystallised sulphide, selenide, and telluride of silver, and of filiform silver, by M. Margottet. The former are obtained by passing vapours of sulphur, selenium, and tellurium, over silver (heated red) by means of a current of nitrogen.—The silver gets covered with the crystals. The crystallised sulphide is transformed into metallic and filiform silver by a current of dry hydrogen at 440 degrees. The corresponding reduction of the selenide and telluride takes place only at the highest temperatures the glass can bear.—Use of lacs of eosine and fluoresceine for preparation of decorative paintings without poison, by M. Turpin. A potassic or sodic solution of eosine, *e.g.*, treated by an acid, gives a precipitate of eosic acid insoluble in water; this washed till the water begins to take a rose-colour is insoluble in the hydrate of oxide of zinc, and so forms a very rich lac (eosinate of zinc) varying from rose to deep-red, according to the quantity of eosic acid used.—Vine districts attacked by phylloxera (1877), by M. Duclaux. It is noted with reference to L'Aude that the vineyards bordering on the sea (some kilometres in width) are preserved much longer than the others.—The natural enemies of the phylloxera in Germany, by M. Blankenhorn. The small extension of centres of phylloxera there is attributed to the fact that the stocks have been frequented, previous to the phylloxera infection, by natural enemies of the

insect (which are specified).—On the intermediary integrals of the general equation with partial derivatives expressing that the problem of geodesic lines, considered as a problem of mechanics, supposes a rational integral with reference to components of the velocity of the moving body, by M. Lévy.—Calculation of the longitude or the hour of Paris at sea, by occultations of stars, by M. Baills.—On the conditions with limits in the problem of the elastic plates, by M. Boussinesq.—On the equation of Lamé, by M. Brioschi.—On apparatus for projection with polarised light, by M. Laurent. For polarisers the author uses Nicols made of two, three, or four pieces of spar, each piece with two faces cut parallel to each other and the cleavage; then he cements them together with a hard mastic, and operates the whole like a single piece of spar. The analyser used is a Nicol of 22 mm. diameter; it is placed at the principal focus of the projection lens.—Action of oxychloride of carbon on toluene in presence of chloride of ammonium, by MM. Ador and Crafts.—Action of stable anhydrous acids on stable anhydrous bases; explosion of the compound, by MM. Solvay and Lucion.—Anhydrous phosphoric acid and oxide of sodium may exist intimately mixed in fine powder without reacting at the ordinary temperature, but a rise of less than 100° causes instantaneous combination with remarkable violence. M. Lucion sees here no confirmation of the dualistic theory or argument against the unitarian.—On the sensibility of the pericardium in the normal and pathological states, by MM. Bochefontaine and Bourceret. The sound pericardium is sensible; the external face apparently more than the internal. The sensibility can be shown by mechanical excitations. The pericardium inflamed experimentally shows a lively sensibility, at least on the external face and inwards.—Maturation and diseases of the cheese of Cantal, by M. Duclaux. Enough water remains for development of ferments, and there are present lactic acid and albumen.—Observations on the zoological affinities of the genus Phodilus, by M. Milne-Edwards.—On the measurement of the dihedric angles of microscopical crystals, by M. Bertrand.—On the signification of various parts of the vegetable ovule, and on the origin of those of the seed (concluded), by M. Baillon.—Preparation of alcoholic chlorides and their application to the production of colouring matters, by MM. Monnet and Reverdin.

December 24, 1877.—M. Peligot in the chair.—The following papers were read:—On some applications of elliptic functions (continued), by M. Hermite.—On the rotatory power of metastyrolene, by M. Berthelot. Metastyrolene, derived in the cold state from active styrolene, has rotatory power (just as metaterebenthine shares the rotatory power of terebenthine). On the other hand, inactive styrolene, as prepared by the pyrogenic method, gives an inactive metastyrolene.—On Saturn's ring (continued), by M. Tisserand.—Observations on the Bahmié cotton plant, by M. Naudin. This plant, which is found in Egypt, and is very productive, is not, as supposed, a cross between cotton (*Gossypium*) and Gombo (or *Hibiscus esculentus*). It differs from the old race merely in aspect. It is still in process of improvement. The botanical species is the *Gossypium barbadense* of Linnæus and Parlatore, or Sea Island cotton. It requires much heat, and is recommended for Algeria, where the cotton-industry has been declining.—Notions concerning intramolecular work (continued), by M. Boileau.—On M. Boiteau's recent communication regarding comparison of the phylloxera of the oak with that of the vine, by M. Balbiani.—On anthogenesic Homoptera, by M. Lichtenstein.—Progress of the phylloxera in the south-west of France, by M. Duclaux.—On the results obtained by use of sulphide of carbon for destruction of phylloxera, by M. Marion. Reiterated treatment with small quantities is recommended.—On the ventilation of the transport-ship *Annamite*, by M. Bertin. After three hours the volume of air evacuated under the sole action of heat from the chimneys was over 29,000 cubic metres, and might rise to 40,000. This movement of air would secure a renewal of air in the hospital about eight times in the hour.—The death of Ruhmkorff was referred to.—M. Dumas, *à propos* of MM. Cailletet and Pictet's almost simultaneous success in liquefaction of oxygen, read a passage from Lavoisier, showing he had anticipated such results.—On the condensation of oxygen and carbonic oxide, by M. Cailletet. His method was to expand the gases suddenly; when cooled to − 29°, and compressed to the extent of 300 atmospheres; a thick mist appears. This is had from oxygen, even at ordinary temperature, if it have had time to lose the heat acquired simply through compression. Hydrogen, under similar treatment, gave no such mist. Nitrogen was not experimented with.—Experiments by M. Pictet on liquefaction of oxygen, by M. de Lognes. The apparatus is described.—M. Dumas opened a sealed letter deposited by M. Cailletet on December 3, announcing his discovery. M. Pictet's results were announced on December 22. Several members expressed opinions on the subject.—New observations on the *rôle* of pressure on chemical phenomena, by M. Berthelot. He calls attention to the fact that the decomposition of chlorate of potash into oxygen and chloride of potassium an exothermic reaction, and not limited by its inverse, is not stopped by a pressure of 320 atmospheres.—On the employment of graphic methods in the prediction of occultations, by M. Tissot.—On the transformations of contact of systems of surfaces, by M. Fouret.—Experimental researches on magnetic rotatory polarisation; magnetic rotations of luminous rays of various wave-lengths, by M. Becquerel. He experimented specially with bichloride of titanium, interposing a spectroscope between the eye and the analyser in his former apparatus; and he notes some differences between the positive and negative magnetic rotations.—Ordinary and extraordinary indices of refraction of quartz for rays of different wave-lengths as far as the extreme violet, by M. Sarasin. The numerical results for lines of cadmium, sodium, zinc, and aluminium are tabulated.—Engraving on glass by electricity, by M. Planté. The surface of a plate of glass or crystal is covered with a concentrated solution of nitrate of potash (poured on it). A horizontal platinum wire, connected with one of the poles of a secondary battery of fifty to sixty elements, is placed in the liquid along the edges, then holding in the hand the other platinum electrode, covered, except at the end, with insulating matter, one touches the glass with it, and draws characters, &c., which remain distinctly engraven.—On acid acetates (continued), by M. Villiers.—On experiments showing that meningo-encephalitis of the convexity of the brain produces different symptoms, according to the points of this region that are affected, by MM. Bochefontaine and Viel.—On the conditions of development of Ligulæ, by M. Duchamp. He made two pigeons (which are pretty far removed from aquatic birds), swallow some ligulæ from a tench. After four and five days respectively they were killed, and each had in its intestine a living ligula with genital organs developed, and the matrices full of eggs (just as with the duck).—On a miocene alios in the neighbourhood of Rambouillet, by M. Meunier. This points to sudden cataclysm.—On thermal coloured rings by M. Decharme. He remarks on the difference in these on tinned and on zinked iron plates.

VIENNA

Imperial Academy of Sciences, November 8, 1877.—On the least absolute number of sound-impulses that are necessary to production of a tone, by M. Pfaundler.—On generalisation of known triangle propositions to any perfect *n* angles inscribed in a conic section, by M. Cantor.—On the perfect square in general, by the same.—On citramalic acid, by M. Morawski.—On accessory projections in the skull of leporides, by M. Mojsisovics.—On arbitrary and spasmodic movements, by M. Brücke.—On cork and corked tissues generally, by M. Höhnel.

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